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ACHOLIC DISEASES;

COMPRISING

JAUNDICE, DIARRHŒA, DYSENTERY, AND CHOLERA.

ACHOLIC DISEASES;

COMPRISING

JAUNDICE, DIARRHŒA, DYSENTERY, AND
CHOLERA.

With a Preliminary Dissertation on
BILE, THE BILIOUS FUNCTION, AND THE ACTION
OF CHOLAGOGUES.

BY

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INSPECTOR-GENERAL OF HOSPITALS,
IN SLIGHT TESTIMONY
OF RESPECT FOR HIS GREAT PROFESSIONAL EMINENCE,
AND IN GRATEFUL ACKNOWLEDGMENT
OF KINDNESS RECEIVED,

This Volume is Inscribed

BY HIS MUCH OBLIGED AND FAITHFUL SERVANT,

THE AUTHOR.

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When present (that is, in the atmosphere, or supra-tellurial) it is attracted, and more or less neutralized, by every, even the healthiest, human body. In certain states, however, of the body, varying from health, the force of attraction is stronger, and the process of neutralization also more active and conspicuous. So long as the vital powers are sufficient to keep up this process of neutralization, and so preserve an equilibrium, the system is not overcome by the disease. But, under unfavourable conditions, the powers of life are not adequate to carry on, through the lungs, a continuous process of neutralization. The poison is then, of necessity, conveyed through the lungs into the blood. Being there, a far more energetic process for its neutralization is required. A violent convulsion of nature ensues, and the symptoms of "Cholera" are present pp. 143—168

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INTRODUCTION.

THE beginning of the present century witnessed the apparition of a new and terrible disease. Whilst we watched in silent awe, but still in fancied security, its ravages in a distant quarter of the globe, and saw it decline, sated and exhausted—worn out apparently with its own devouring energy—already its unsuspected march in our own direction was commenced. Scarcely had Hindoo mythology numbered another among its most potent and dreaded deities, when Christian Europe turned aghast to science, in the hope to find some definite knowledge, some tangible conception, of this new and unseen power, this “pestilence that walketh in darkness,” whose movements are so mysterious, whose advent in the midst of us is wont to be so sudden, and whose stroke is more deadly and swiftly fatal than that of any other malady that history has recorded—a form of approach more appalling than the king of terrors had ever before assumed.

For the third time in less than half a century general attention in our own country has been directed, with a surpassing intensity of interest, to this now familiar, but no less dreaded foe, whose movements, uncertain and capricious though they seem, advancing and receding alternately, like the swaying waves of a rising sea, former experience forbids to regard but with the utmost apprehension; whilst with the majority of the profession this feeling amounts to a conviction that, in a shape more or less formidable, we shall shortly have to do battle with it here at home.

To them a strenuous appeal on the part of the public, through the medium of the press, has already been made; and assuredly not in vain. Throughout the land the most earnest and thoughtful among us have been engaged in considering how the coming contest will best be waged. The fruits of former experience, the stores of modern science, and the past thinkings of able men, are all being brought once more to the light, anew to be reviewed, tested, and compared.

But in the midst of this general appeal from the public to the profession, there has arisen one voice,* solitary but strong, of reproach — reproach that our profession has not sustained that high place that our own arrogance had assumed, and that a generous public had conceded—reproach of an

* *Vide* "Times."

absence of method and logical accuracy in our inductions ; of inconsistency in our practice ; of disagreement amongst ourselves on the first principles, vital and fundamental, of our art. Abler pens have already responded to this undeserved attack ; and I only allude to it here to take the opportunity of pointing out, that a special occasion is probably now at hand, when we shall be enabled by our acts to refute what has been already, and eloquently, disclaimed in words—that part at least of the reproach that taunts us with a want of consistency.

Besides those more obvious sanitary and prophylactic precautions that are demanded for the conservation of a high standard of public health, and of that individual strength that will best enable each one of a community to resist and cope with a surrounding morbid agency, it seems to me much to be desired, that members of our profession generally should have recourse exclusively to such curative means as will accord with their own individual preconceived and settled views of the pathology of disease ; so that theory and practice may be *seen* to go hand in hand. Let the essaying of far-fetched and preposterous remedies be left to empirics, who act under the sordid hope of luckily lighting on a specific that may in a twinkling elevate to fame and fortune—*sic itur ad astra* ! Even such subordinate and grovelling agencies may not be altogether without their use—*melius aliud*

agere quam nihil—and wisdom may sometimes not unprofitably mark the blundering steps of ignorance.

It has always seemed to me that the practitioner, who has an educated and intelligent patient, should be prepared to furnish to him, without technicality, a tolerably clear account both of his disease, and of the principles on which he is being treated. I would not, of course, be understood to imply that such an exposition is in all, or most cases, necessary; but that the consciousness of an ability to do so will supply to the practitioner's own mind satisfactory evidence that he is himself acting on common-sense—not to say rational—grounds. What then each individual member of the profession should be able to offer to his own patient we should, as a body, be able, if called upon, to answer to an intelligent public. We should be able to boast that, in our pathology, as well as in our treatment, we pursue those patient and laborious processes of investigation common to, and indispensable for, all scientific research; guided ever by those acknowledged rules of inductive reasoning that have their foundation in immutable truth.

In an article published in the *Lancet* of the 7th October, 1865, I attempted to lay before the profession an epitome of my own experience and views regarding the pathology and treatment of Cholera.

But in the endeavour thus to confine my remarks within the prescribed limits for publication, I felt throughout that I was labouring under a twofold disadvantage; that, for want of sufficient amplification, I was incurring the risk of being misapprehended, or only imperfectly understood; and, in default of sufficient illustration, of my views being deemed too crude and speculative for practical adoption.

Besides, in the endeavour to reduce to order and precision the ideas that my mind had in the course of many years—instinctively, as it were, and by an almost imperceptible process of reasoning—imbibed, I found, as many a one before, I suppose, has found (under like circumstances), that these ideas were by no means so definite and distinct as I had imagined; that, though leading principles might show, clear and prominent enough, subordinate details had to be compared, tested, and worked up into relief. As also might have been expected, I found, in the course of my inquiry, that new channels presented themselves for investigation—suggestions that needed scrutiny, and objections that called for reply.

Amongst the principal of these fresh subjects of inquiry was a degree of affinity, or relationship, that I fancied might be not unprofitably traced between cholera and certain other diseases of the same organs, the investigation of which belief resulted in

the addition of the chapters on Diarrhœa; and Dysentery, both Acute and Chronic.

Again, another addition has been found scarcely avoidable in the form of two chapters on Bile and the Biliary Function. In the course of my inquiry I found that in connexion with, and in support of, the views I desired to illustrate, a reference to the operations of those viscera more immediately and obviously involved in cholera, was of such frequent necessity as materially to interrupt and thereby confuse the even tenor of my theme. To remedy this, I was at first tempted to transfer all such inconvenient interpolations to the margin in the form of notes. But frequent, and still more lengthy, notes have always appeared to me but clumsy expedients for associating what the art of the writer has failed harmoniously to incorporate. Nothing can be more trying to the patience of a confiding reader than to be carried along unwittingly through a note of some pages, and then to have, in sporting phrase, to "hark back," for the purpose of hunting out his forsaken text, and resuming, as best he may, the interrupted current of his thoughts. I therefore resolved on attempting to collect these cumbrous and unavoidable digressions, and to arrange them into one connected whole, such as would form a convenient and natural introduction to the main topic.

In reviewing, however, these annotations for that

purpose, I found that by far the largest proportion had reference to the liver, and to the part that great organ performs in health and disease, more especially in connexion with the pathology of cholera. But as it was no part of my plan to enter into a circumstantial account of the structure of the liver, with which I must suppose my readers sufficiently acquainted, but only to make such general reference to this from time to time as occasion might demand, I found my ground of inquiry thus conveniently narrowed to an investigation of its function, and the product of that function, bile. Other and important viscera are, of course, implicated in cholera; indeed, it would be difficult to indicate any particular structure that is not. I need not now enter on this interesting question; nor will I anticipate my future subject by considering whether the liver be even the organ primarily affected. It may be deemed sufficient here to suggest, that though our acquaintance with the minute structure of the liver is now probably as complete as we possess in respect of any organ of the body, our knowledge of its function, for reasons I need not detail, is comparatively doubtful and imperfect, particularly when we consider its superior size and manifest importance in the economy. Preparatory, therefore, to the study of a disease in which the suspension of the hepatic secretion is one of the most prominent and constant features, it

did seem very desirable that, if possible, some definite conception should be established respecting that function. In this was necessarily involved an examination of the nature of bile, its use in the economy, and the changes to which it is liable. And since, with this view, a very large proportion of the notes, as already stated, had had reference to the same subject, it seemed most natural and convenient to combine them, so as to form the two chapters alluded to on Bile and the Biliary Function. And in connexion with the same topic, a third short chapter has been introduced on the action of Cholagogues. Finally, and as a sequel to the subject of cholera, a few suggestions have been appended "On Sanitary Conditions." It is hoped that these various additions will not be regarded as superfluous; but rather that some of the questions from time to time arising may serve to interest, even if the arguments adduced should not serve to convince, the reader.

Thus I have endeavoured to account for the origin and growth of the present treatise. And all imperfectly as I am sensible that my purpose has been carried into effect, I throw myself on the indulgent perusal of my brethren, trusting that my having attempted the task—*pro tenuitate mea*—will, at least in part, be excused on the following grounds.

In the first place, I think it will, without much

hesitation, be conceded that, with regard to the nature, pathology, and treatment of cholera, a remarkable want of unanimity prevails. Of its specific nature, including its mode of progression, communication, increase, and exhaustion in a community, I think I may venture to assert that no consistent theory has ever been broached; affirming this, on the broad ground, independent of reasons in detail, that no one of these theories has stood the test of accounting for *all* that has under those heads been observed; whilst the same objection applies to the conflicting views respecting its pathology, these being singly insufficient to account for the whole of the objective symptoms, which in this, above all other diseases, are marked and constant; differing only, as I hope to show, in intensity.

But when we come to the treatment, a still greater perplexity and confusion are conspicuous. It is not so much that every known remedial agent of any power has been tried in succession, and successively vaunted as possessing supreme claims, and a special virtue in control of the disease; this perhaps is incidental to the practice of an art so uncertain and so necessarily tentative as medicine, and not peculiar to the treatment of cholera. Besides (as before observed) that the results of even such indiscriminate and empirical practice are doubtless being carefully noted by the more

•

sagacious and reasoning few, and by them utilized to a more perfect knowledge of the subject.

It is not, then, so much this variety of treatment that I refer to, as to a want of concord and logical relation between treatment and pathology—that these various remedies have often no necessary and obvious connexion with some special pathology entertained by those who use them, or with any pathology whatever; but are resorted to tentatively by one, because in the hands of another a certain proportion of cases has, under such means, got well; seemingly unmindful of the fact that an equal success, and under equally high authority, has elsewhere been recorded under a totally opposite treatment—nay, that possibly quite as many would have recovered without any treatment at all.

To the younger, therefore, of my professional brethren I would respectfully tender the counsel not to allow themselves to be fascinated by brilliant results recorded on paper, and to be especially mistrustful of figures and facts so called. These last are not nearly such stubborn things as is popularly supposed, but accommodate themselves with a marvellous pliancy to foregone conclusions. I would remind them that the noblest men of our profession in this country—those great and familiar names that stand out on the historic page with a lustre that renders needless enumeration—those lights of science have not much troubled the world with

figures. For their proofs they have resorted to well-known and admitted facts, or to such within their own knowledge as allowed of personal experiment and verification on the part of their readers; and as for their convictions, the slow growth of multiplied experience conjoined with profound powers of observation, these have been always stated broadly and boldly, unbolstered by figures, and bearing on their face the unmistakable impress of united genius and truth.

I would, therefore, counsel them to be servile followers of no self-constituted authority—*nullius addicti jurare in verba magistri*—but to endeavour, in the treatment of this, as of all other disease, to form a clear and distinct conception of the object they have in view, of the means they propose to employ, and of the physiological grounds on which their expectations of success are based. Even if wrong, the notion of being right imparts to a man's actions a certain vigour and decision that is in itself a very important element of success. "Let every man," says St. Paul, "be fully persuaded in his own mind"—advice no less sound in its application to secular, than it is to spiritual, matters.

To prevent, therefore, any misconception as to the purport and scope of the present work, I may as well, *in limine*, premise that I have no new remedy to propose, no new special treatment to recommend, which is to be persisted in under all

conditions, and in every aspect, phase, or stage of the disease. My object—and I imagine it is my duty as well, for *qui novit, neque id quod sentit, exprimit, perinde est ac si nesciret*—is rather to lay before the profession the results of upwards of twenty years' experience and attentive observation of cholera and kindred diseases in India, with the conclusions I have arrived at respecting the rationale of treatment that it will be most prudent and safe to adopt; leaving it open to myself as well as to others to have recourse to any remedies that may be used in consisteney with those general indications, with an assumed pathology of the disease, and, above all, with the curative process of Nature herself.

Nor, perhaps, should I have considered myself sufficiently warranted, by the circumstance alone of having seen much of disease, to obtrude my observations thereupon, were I not at the same time impressed with a belief that some of my views, whether true or otherwise, have at least novelty to recommend them to the curiosity of the reader. From this character of novelty it also naturally follows that I have not often had occasion for, or possessed the advantage of, quoting the opinions of preceding medical writers, my conclusions being destined to stand or fall on their own merits; whilst, on the other hand, I have not hesitated to select, wherever I could find them,

such authentic and commonly received facts as seemed concurrent with my own experience, or corroborative of the views I entertain—*quod hausi concoquens*.

I might, indeed, have quoted several facts and observations within my own personal experience that have, in the course of many years, contributed to the development and formation of the theoretical views that I now submit for the judgment of the profession; but I think it will be found that I have consistently refrained, when engaged in such discussions, from adducing these or any other data, that rested solely on my own evidence, or on that of individuals. For I hold that, in the establishment of any general law in natural science, the propounder is bound to use such arguments only as can be referred to universal experience and testimony. It is also evident that rare and exceptional circumstances, even when of undoubted authenticity, are admissible in illustration only, not as demonstrative. I have accordingly confined myself, in such inquiries, to those familiar and generally admitted phenomena, that are within the scope and knowledge of every well-educated medical reader.

It would also have been easy, availing myself of precedent and custom, to have swelled this little work to double its present size, by a “judicious” selection, at the end of each chapter, of “cases.”

But my aim has been rather to condense than to amplify ; and I imagine the reader will not be slow to excuse this omission.

Further I would remark, with respect to many of the theoretical views propounded in the following pages, that they are submitted with a perfect sense of their manifold defects, and palpable points of weakness. But at the same time I would venture to suggest to my more critical readers, that no "theory," properly so termed, can be expected to stand the test of absolute analysis, or can be founded wholly on *facts*. "Theory," indeed, has its perfect synonyme in "speculation." And a "theory" based absolutely on facts would cease to be a theory—it would be a truth. But when, by means of a theory, there is furnished a consistent way of accounting for phenomena, whose real nature and origin is obscure, such a theory may be conveniently adopted, in the absence of more exact knowledge. A theory may be even manifestly untrue, and yet of great practical value. No one supposes that the atomic theory is true, much less founded on ascertained facts ; but it is nevertheless adopted universally for its convenience and simplicity, and for its apparent consistence with known phenomena.

Theories are indeed the inseparable accompaniments of an inexact science, such as medicine. By their judicious selection, however, and condi-

tional adoption, and by keeping them steadily in view, the physiologist is enabled to practise his art, not blindly or empirically, but on principles that are at least intelligible, and that admit of definition. As already said, a theory must be regarded as a substitute only, and preliminary to more exact knowledge. With the growth of a science, theories once accepted as plausible are laid aside—a temporary scaffolding, to be taken down as the building itself acquires strength and permanence. A striking illustration may be found in the history of geology, and in its development as a science, within our own times.

I would express, therefore, a hope that, in the event of my failing, as is but too probable, to convey conviction, something of what I have thought and said may haply be found suggestive, and thus prove in other hands instrumental to the further advancement of truth. I would fain, also, think that other and more cogent arguments in support of my own views, and other and more striking illustrations, may not impossibly occur; whilst it need scarcely be added, that any such aid will ever be gladly recognised, and gratefully acknowledged.

From the *prospective* tenor of the above "Introduction," and from corresponding remarks in the chapters on Cholera, it will be seen that they

were penned some time since, and whilst the advent of cholera was still a contingency only. But publication has been delayed by want of leisure, in consequence chiefly of domestic circumstances, that called for all the author's attention. A condensed sketch, however, of his views on the Nature of Cholera having appeared as far back as October, 1865, in the pages of the *Lancet*, the article containing them is appended at the end of the work.

CHAPTER I.

ON AN ENTERIC SECRETION OF BILE.

- I. Important distinction between Liver and all other Glands in its Blood-supply—Bile contrasted with Urine—Fallacy of any supposed Analogy between them—Portal System, its Relations considered—"Cholerized" Blood—Pathology of Hæmatemesis and Melæna—Hyper-Cholerization—Organs chiefly affected thereby.—II. Means of estimating the Biliary Secretion from aspect of Egesta but limited.—III. The Liver not Subsidiary to the Lungs.—IV. Results of Hyper-Cholerized Portal Blood—Constipation not a Result—True nature of Constipation, its Course, and natural Cure—Diarrhœa a Secondary Result.—V. On an Enteric Vicarious Secretion of Bile—Arguments in its favour—Serves to Explain various Anomalies in Pathology and Therapeutics—"Achoelia"—If other Tissues can Secrete Bile, much more the Intestines—How a Purgative, or Cholagogue, Cures a Diarrhœa—Interpretation of Dark-green or Olive-coloured Bile in the Motions.—VI. Source of Enteric Bile—Rudimentary Position of Liver—Intestinal Glands in effect Rudimentary traces of Liver—Opinion of Abercrombie respecting "Bilious" Diarrhœa—Does the Pancreas ever secrete Bile?—Instances of Vicarious Secretion in Disease.—VII. Illustrative Cases.*

I. THOUGH the physiological arrangement for the secretion of bile bears a common resemblance to that which obtains in other secreting organs, there is one secondary, but, at the same time, very striking difference that distinguishes the liver from every other glandular structure, consisting in the

anomalous method by which, through the great portal system, it is supplied with blood.

Hence it follows that, whatever general laws we may think fit to establish regarding the functions of these organs, classing them all under one common denomination of glands, and whatever consequences we may deduce from the operation, suspension, or derangement of their respective functions, cannot be expected to apply altogether to the liver, but must be modified to suit this peculiarity.

Amongst other physiological consequences of this anomalous blood circulation, there is one in particular to which I would direct attention ;—in the first place, because it seems to me of much interest and importance in its connexion with disease ; and secondly, because, so far as I am aware, it has not received special notice at the hands of any previous medical writer.

For this purpose it may be worth while to compare, or rather to contrast, the secretion of bile with that of the urine, which by many writers is familiarly spoken of as an “analogous” product. With respect to this assumed analogy, I can only here passingly indicate the manifest inexpediency of associating, for any purpose of physiological or pathological inquiry, a fluid such as bile, that is destined to serve certain ulterior and beneficial uses in the economy, with another that is positively noxious, and the entire removal of which out of the

body is a matter of urgent and vital necessity. But it is not to this difference, which, however important and essential, is still sufficiently obvious, that I wish more particularly to allude.

In the case of the urine we know that the blood, more or less charged with the materials for its composition, enters, by the emulgent arteries, the kidneys, and pereolating them, passes on, by corresponding veins, into the general circulation. In the course of this transit through the kidney, should urine have been eliminated in excess, or scantily, or in any other way varying from health, the consequences are, so far as we know, or can reasonably infer, distributed equally, by means of the general circulation, to all parts of the body. No one organ in particular is immediately and necessarily affected more than the rest, by these variations in the working of the kidneys. Not of course but that, in renal disorder, the signs of secondary disturbance may be recognised earlier in one organ than another; but then this is not owing to that particular organ being subjected to a greater modifying influence through the medium of the blood, since this fluid circulates equally amongst them all.

The same reasoning applies to the functions of the pancreas, testes, mammæ, salivary, and all other glands, whose mechanism of blood-supply is on the same general principle that obtains in the kidneys.

But the provision of blood for the due discharge of the hepatic function is on a totally different plan; and we might accordingly expect to find that the secondary consequences arising from variations in this function should correspondingly vary. One important departure from the general law just quoted I will endeavour to show.

The great portal system may in one sense be considered analogous to the emulgent artery, inasmuch as it supplies to the liver the blood from which bile is eliminated, just as the artery supplies to the kidney the blood from which urine is eliminated. At the same time it is, as we know, unlike any artery in its origin, course, and general distribution. To create this system all the organs engaged in the work of digestion may be said to contribute—the stomach and intestines, mesentery and omentum, pancreas, spleen, and gall-bladder. For the sake of simplicity, however, we will consider it merely in its relations between the liver on the one hand, and the stomach and intestines on the other.

Arising then from the alimentary canal by innumerable tributaries, and terminating at the liver in a single trunk, it is thus completely included, and, so to speak, isolated, between these great boundaries.

In this mass of portal blood, thus in a manner isolated from the general circulation, are stored the elements required for the composition of bile. Of

course not, under ordinary circumstances, is bile itself to be found there. Nor would I imply that these elements for its future fabrication, singly or collectively, bear any resemblance to bile, or possess any of its sensible properties.

The materials, then, of bile being thus included within the portal system, it follows that any sensible properties that these elements possess must affect, if not exclusively, at least pre-eminently, the organs or parts with which that system is in contact, assuming these to be (as already agreed upon) the liver on the one hand, and the whole alimentary canal on the other. No other viscera or structures, besides those engaged in the general process of assimilation, can be so directly affected; nor indeed at all, until the portal blood has been first more or less modified in its passage through the liver.

Here, then, we recognise another remarkable departure from any analogy that may be assumed to subsist between the portal circulation and an artery—as the emulgent, for example.

If the blood in that artery, in its passage through the kidney, be obstructed by congestion of that organ, or any other cause, the secondary condition of fulness or congestion, that would otherwise ensue within the artery itself, is provided against by its free communication, retrogressively, with the aorta and general circulation. The first viscus that

would feel the consequence of this renal obstruction would be the heart itself; and before the effect was communicated so far, it would be so relieved and attenuated as to be scarcely appreciable; in other words, the effect would be distributed throughout the entire circulation. It is far different when the portal circulation is obstructed.

Again, if the constituents of urine with which the emulgent artery is charged be unduly or imperfectly excreted by the kidney, this effect too is shared equally by the whole circulation. There can be no tendency to a greater accumulation of these urinary constituents within the limits of the artery itself; for, before arrival there, they have been collected from all the tissues of the body, carried through the whole venous and lymphatic systems, and finally emptied, through the heart itself, into the aorta. Very different are they in this respect from the materials of bile; which, if not actually prepared, as is universally supposed, by the alimentary canal, must there undergo final and characteristic modification, and are carried thence, *direct*, and by the shortest possible course, to the organ which is to extract and combine them.

For this condition of the blood, peculiar, at least in a state of health, to the vena porta and its tributaries, I would propose, as convenient and significant, the term *cholericized*, meaning literally "flowing with bile," or "in which bile flows."

If, therefore, any impediment occur to the free course of this "cholericized" blood through the liver, the whole portal system must participate in that obstruction; and next to the liver itself, or even in an equal degree, we should expect that effect to be communicated to the stomach and intestines. That this expectation is well founded, we have strong evidence in the occasionally congested state of the hæmorrhoidal veins; whilst a corresponding condition may be reasonably assumed to exist in the upper portion of the intestines, both large and small, and of the stomach itself. It is very probable that this preternatural fulness of the intestinal capillaries sometimes manifests itself, and at the same time is relieved, by hæmatemesis and melæna.

An opposite opinion has indeed been entertained, to the effect that, in cases of vomiting of blood, this fluid proceeds from the liver itself, and is poured into the duodenum through the common duct. But this notion is unsupported by any known facts. That blood is being effused, in cases of internal piles, from the mucous lining of the lower intestine, is a matter of daily observation; and therefore, that blood should similarly exude from the upper part of the same common canal, is only consonant with sense and probability. And when we have this simple explanation, it does seem unreasonable to have recourse to another that is far-fetched, purely hypothetical, and opposed to all analogy.

Again, if the secretion of bile by the liver be inadequately performed, or even suspended entirely, it follows that there must be, *pro tanto*, accumulating in the portal system the elements, not necessarily of bile, but for its constitution; for though the liver may thus diminish or suspend its operation, the intestines may be supposed to continue theirs; and, whenever this is the case—whenever the latter furnish more of the elements of bile than the former can eliminate—the blood included between them, that is, the portal system, must become more and more charged with biliary constituents—more and more *hyper-cholerized*.

It may here be objected, that though all the constituents of bile may not be taken up by the liver, it does not therefore follow that these are retained and accumulated *on this side of it*; they may pass through unmodified, and, by the hepatic vein, entering the general circulation, may be distributed throughout the body. But this can only happen in those cases, extremely rare, in which the action of the liver is absolutely and totally suspended; in the more ordinary instances, in which the liver is only imperfectly or inadequately performing its function, though the whole circulation may thus become partially “cholerized,” the portal blood will always remain so in a still higher degree.

Thus much is certain: that this portal blood, if not duly depurged in its course through the liver,

must assume some abnormal condition, which I would recognise by the term "hyper-cholerized." And if this be the case, it follows, as a corollary, that the organs with which this hyper-cholerized blood is more immediately in contact will be more particularly affected by any new sensible qualities it has thus acquired. And the stomach and intestines being the chief of these, we should expect them to furnish, by subjective symptoms, evidence of being so affected. What those symptoms are will be matter for early inquiry.

Of course it will be understood that other viscera, as the spleen and pancreas, that contribute to furnish portal blood, will be similarly affected by changes therein; but of mere functional, or temporary derangement in these organs we have little direct evidence during life; and a recognition of this collateral element will be more conveniently omitted.

II. Before proceeding further with our inquiry into these retrograde, or retrocedent effects of an abnormal condition of the portal blood on the intestinal tube, there is one point in reference to the secretion of bile on which it will be desirable to establish a clear and just conception, and which does not appear to have been investigated with the minuteness its practical import deserves.

It is generally supposed that by an inspection of

the alvine evacuations we can form a tolerably correct estimate of the state of the liver as regards its proper function; that we can pronounce whether the supply of bile be defective or in excess; and in what degree it departs in colour and other sensible qualities from the condition of health. To a certain extent this is undoubtedly the case; but I would suggest that we are apt to overlook, or at least underrate, certain intermediate agencies to which the bile in its course through the intestines is subjected, and which, by the time it is presented for our inspection, have materially modified both its quantity and aspect.

Thus, in so-called bilious diarrhœa we pronounce that the bile is in excess; in an ordinary healthy evacuation, that it is normal; and when the stools are clay-coloured, that there is little or none. Lastly, when they are absolutely white, as is sometimes seen in children's diseases—or consisting purely of slime and blood, as in dysentery—or of colourless mucus, as in cholera—in all these various forms of disease we alike conclude, because there is no trace of bile in the *egesta*, that there is absolutely none being secreted by the liver. So far, however, from such inferences being correct, or even sufficiently so for practical purposes, I believe they may be, and generally are, very wide of the truth. I would even go so far as to affirm that in the evacuations there may be scarcely visible a trace of bile,

and that nevertheless a comparatively large proportion may be in daily course of secretion by the liver, and duly poured into the duodenum.

A moderate estimate of the amount of bile secreted in twenty-four hours fixes it at one pint—or say one pound; whereas the quantity voided with the fæces, in a condition of health, does not exceed one scruple, or about the three-hundredth part; or, allowing for evaporation, and taking the lowest possible estimate, we will assume that, of the bile daily secreted, twenty-nine parts are re-absorbed, and one only evacuated, or the thirtieth part.

At the same time, we may reasonably conclude that there is nothing constant in this proportion of thirty to one, or in any other proportion. We may also safely affirm that the bile-absorbing power, or tendency, of the alimentary canal is rather regulated by the wants of the system, and not necessarily by the quantity of bile passing through it; that if the bile supplied by the liver should exceed the amount required for the purposes of assimilation, a portion will remain unused, and pass out with the *egesta*; and, on the other hand, if the supply of bile should be defective, or less than is so required, that the whole may be used up before it reaches the lower part of that long canal; and that, consequently, no trace would remain in the fæces. That is to say, supposing in any given stool there were no distinct trace of bile, we could not posi-

tively tell, from that evidence alone, whether, taking the diurnal quantity in health to be one pint, the amount secreted by the liver had been nearer twelve ounces, or eight, or four, or even one.

Much less, then, could we affirm, because none was to be seen, that none had been secreted, and, inferentially, that the liver's function was temporarily suspended. Not that I would be understood to imply that the function of the liver never is absolutely suspended; only that, on many occasions when we are in the habit of concluding that there is very little or no bile being poured into the intestines, there may in fact be a considerable amount; and that, further, the occasions of absolute suspension are much rarer than is commonly supposed, and only to be met with in the severest forms of disease. And though I have dwelt somewhat at length on this subject, I trust hereafter to be able to show, when we come to consider the pathology of cholera and dysentery, that it is one not of curious inquiry only, but of special import as regards the pathology and rational treatment of those formidable diseases.

Any one who has seen the excrements of tigers, hyenas, jackals, and other beasts of prey, could not have failed to notice their chalk-like aspect. This is owing to the entire absence of colouring matter, and, necessarily, of bile. And yet these animals have a liver and gall-bladder, through the medium

of which bile of a bright yellow colour is abundantly supplied. And not only is bile absent from their *excreta*, with its organic constituent, choleic acid, or bilin; but also its other essential element, soda, which, from its inorganic nature, could not have undergone decomposition, and therefore must have been re-absorbed. From this complete absence of bile in the *egesta* of carnivoræ, it would not be much, then, to assume that in man (who, perhaps, without much straining of analogy, might be classed in the same category) a similar complete re-absorption may sometimes obtain, at least as an exceptional or morbid result.

In connexion with this subject it is also worthy of note that fat and soda, two essential elements of bile, are both found abundantly in the chyle; though, as already observed, neither is discoverable in the *fæces*. And further, that in those diseases in which the common duct is obstructed by a gall-stone, or otherwise, the patient rapidly loses his fat, as has been remarked by Dr. Budd, in his admirable work on the Liver, page 29.

III. I cannot help here, at the risk of being considered unreasonably discursive, alluding to another, and, as it seems to me, erroneous conception regarding the nature and object of bile; namely, that its secretion is subsidiary to the function of the lungs. Because bile abounds in carbon and hydro-

gen, and because the elimination of these, the former especially, constitutes the chief object and result of respiration also, therefore it is concluded that the lungs and liver have at least this one object in common. But in refutation of such a theory, it is surely sufficient to suggest that, for every practical purpose, bile is to be regarded as a beneficial product, furnished only to be taken up again into the blood. In other words, that respiration is an *excretory*, whilst the biliary function is a *secretory*, process; the one for the elimination from the system of noxious and superfluous, the other for the supply of beneficial and necessary, elements. In health, at least, and under ordinary conditions, any such connexion between these two great organs is manifestly impossible.

In some structural diseases of the lungs, in the latter stages of which diarrhœa occurs, and apparently with an increase of bile in the motions, some such compensative result may possibly take place. But, independently of the fact already shown, that only a very minute proportion of the bile thus supplied remains unused, it may also be a question whether this colour in the dejections be owing to hepatic bile at all; and whether it may not be due to a vicarious product furnished by the intestines themselves. The possibility of such a subsidiary action on their part, in phthisis and other diseases of the lungs, is not only interesting

to contemplate, but it is further invested with likelihood by the circumstance, elsewhere fully illustrated, that *hepatic* bile never can acquire any irritant property—except perhaps very temporarily, and after unusual retention in the gall-bladder; and that diarrhoea, and every other form of continued enteric irritation, is caused either by insufficient action of the liver, or from the presence of other and irritating matters. And it is more than probable, though hepatic bile cannot be otherwise than bland and innocuous, that a factitious bile, whether furnished by the intestines or any other organs whatsoever, does always, in a greater or less degree, possess an irritant property. In the following chapter, and in the chapter “On Jaundice,” will be found a fuller consideration of these properties of *true* and *false* bile—contrasting them, and showing that jaundice is either a mild or fatal disease, according as it is caused by one or the other of these diffused throughout the blood.

IV. Reverting from this digression, it will be remembered that we left the portal vein and its tributaries surcharged—not necessarily with bile, or even its *proximate* elements—but with the materials for its extraction; the liver having failed wholly or in part to carry out the needful process of their combination. Or, to afford not the smallest room for objection, we will simply assume that, in

consequence of this hepatic irregularity, the portal blood is in some abnormal state, as cannot but be the case. Let us, then, next inquire into the probable consequences of this condition of the portal blood, by which I understand the whole mass of fluid circulating in the portal vein and its tributaries.

I hope I have succeeded in demonstrating that, setting aside the liver itself, the primary effect must be exerted on the alimentary canal. For the sake of simplicity, and that our knowledge of the variations in the *quality* of bile is so very limited, it will be as well to restrict ourselves to the question of its *quantity*, assuming that morbid results are attributable to this element of variation alone. Nor in this assumption do I think we shall err very wide of the truth, the reasons for which belief I shall take a future opportunity of stating in full.

And first in order let us inquire, *Is constipation one amongst the effects of hyper-cholerized portal blood?*

I need scarcely remind the reader that by *hyper-cholerized* I would imply that condition of the portal blood consequent on insufficient action of the liver. The term "*hyper-hepatized*," on the other hand, will serve to signify an opposite condition, in which the system may be supposed, by excessive action, to be drained, as it were, of its biliary constituents.

Before proceeding to consider the question above

propounded, I may as well, *in limine*, briefly state my own opinion. This is, that hyper-cholerized portal blood, though often and necessarily associated with constipation, so far from being a cause of it, tends, by its irritating quality, to *stimulate* the torpid bowels to pour out their proper secretions, and by that means to *relieve* their own constipation. Thus, hyper-cholerized portal blood may be a proximate cause of diarrhœa, dysentery, cholera—but never of constipation. I now proceed to illustrate this opinion in detail.

Limiting the import of the word “constipation” to a sluggish condition of the digestive canal (*torpor intestinorum*), I imagine that universal opinion ranks among the chief causes of this condition an inadequate secretion of bile. Insufficient action of the liver, then, induces two independent results—on the one hand, hyper-cholerized portal blood; on the other, a sluggish condition of the bowels. But these two *results* have to each other no necessary relation of cause and effect.

Owing, moreover, to this imperfect action of the bowels, the portal blood must be furnished to the liver in some correspondingly abnormal condition. Another element of disturbance is thus introduced. And, in accordance with a familiar law of nature, by which effects sometimes take the place of causes, and causes of effects, these two great functions of the liver and bowels mutually react on one another;

and, during the continuance of this reaction, the blood circulating between must be acquiring a still more abnormal condition—becoming more and more *hyper-cholerized*.

Thus Nature appears to be adding to her difficulties. But at the very time she seems to be so doing, she is in truth paving the way, *accumulating* the means, and providing for a restoration to health.

For it is not until a certain amount of hyper-cholerization is effected that the portal blood acquires a sufficiently irritating quality. When this is attained, it begins to react upon and irritate the bowels. These being thus roused and stimulated, commence to pour out their proper secretions more or less abundantly, according to the degree of irritation or urgency of the occasion. By this flow the constipation, primarily, is relieved; secondarily, the congested veins of the portal system, by exudation of their fluid contents, are relieved; and lastly, the liver itself, partly through relief of this venous congestion, but probably still more so by nervous sympathy with the intestinal irritation, now *happily* begins to act. I say happily; for, until it does, there can be no thorough cure, and nature's efforts have been as yet in vain.

Should it be so, the hyper-cholerized state of the blood is, by the action of the liver, corrected; and, this proximate cause of irritation removed, the bowels subside into the gentler action compatible

with health; whilst simultaneously the bile wells forth, and flowing with unctuous softness over their ruffled surface, mingles with kindred fluids to form the pabulum from which is derived the blood-renewing chyle; at the same time supplying to the intestines that gentle and modulated stimulus that a perfect discharge of their function requires.

Imagine thus harmony restored; but though I have endeavoured, with a prolixity perhaps that has tried the patience of the reader, to portray one of the most common, but not therefore less interesting curative processes of nature, I am sensible how imperfectly it has been done. The complex nature of highly-organized man does not admit of such a simple tracing of cause and effect. Rather might we view that organism in the light of an infinite assemblage of circles, every one of which touches at one or more points *all* the rest. No one organ, no one function, but is connected, directly or indirectly, with all the others; now acting, now being reacted upon, with an intricate and endless succession of change, that none but the eye of Omnipotence can trace, or the same creative hand direct. I have essayed to account for one phase of abnormal action, as if it were a series of simple and unvarying phenomena; but it must be left to the physiologist to supply what is defective, and to imagine for himself those countless modifications to which every vital movement is subject.

To resume: if, as already said, the liver now happily begins to act simultaneously with the intestines, *their* previous constipation is relieved in the easiest and most natural way, and is replaced by the moderate action of health. On the other hand, should the liver fail, or delay to act, or act insufficiently, the bowels, still further irritated by a continued contact with hyper-cholericized blood, make still greater efforts for relief; they pour out their secretions more and more abundantly, and thus the constipation is replaced by a diarrhœa. It is evident that for the rectification of this morbid action there is still one thing wanting; that is, a sufficient resumption of the liver's action, with a sufficient supply of bile; objects to be attained, either by nature's unaided efforts, or to be accelerated by art. The various means to this end, furnished by the latter, will be appropriately considered in a study of the pathology and treatment of diarrhœa in a future chapter, confining ourselves here to a simple observation of nature.

The suggestion will probably here present itself, that simple diarrhœa, in default of a natural cure, is not the only alternative; that this form of constipation, *torpor intestinorum*, may be succeeded and replaced by other secondary results. To a consideration of these I shall also revert hereafter, pursuing for the present the further possible course and consequences of this assumed form of diarrhœa,

in the absenee (it will be understood) of interference by art.

V. We will suppose, then, that the liver continues inert, acting insufficiently, as in some forms of diarrhœa; or, possibly, not at all. Under such condition the following considerations present themselves :—

If, for a restoration of healthy intestinal action, a sufficiency of bile be indispensable, are there, in its absenee, any *palliative* means to which nature may resort?

Is there any substitute for bile?

How long may the vital functions endure without it, or with how small a portion of *efficient* hepatic tissue remaining?

Is every yellow tinge imparted to the *residua* of digestion (as in some diarrhœæ for instance) owing to bile—genuine *hepatic* bile?

By these various inquiries, and especially the last, the reader will perhaps have anticipated the particular point to which they all tend; and that I have attempted to express in the following general proposition :—

Have the intestines any faculty of supplying, either from their own proper mucous lining, or through the secreting glands with which they are furnished, or from both, bile, or a fluid resembling bile in its sensible properties, and therefore in its use in the economy?

When the function of the kidneys is suppressed, we know that a vicarious excretion is substituted. If the essential principles of urine can thus be formed independently of the kidney, may not those of bile independently of the liver—if not bile itself, something resembling or akin to it?

Consider once more the relations subsisting. The liver, inactive, on the one hand; the intestines on the other; between them isolated, and in a manner penned in, a distinct circulation, the portal, teeming with the elements of bile. Will not all our knowledge of nature's method of working under like circumstances, all analogy, indicate a probability that the intestines will take on a vicarious action, and secrete *something like bile*? Like many other truths in our uncertain art, it can only be proved inferentially; and to do so, I must trust less to any power of demonstration than to an intuitive perception, on the part of the student, of a physiological truth. I hope to show also that its admission will serve to reconcile some apparent anomalies, not only in the pathology of disease, but also in our treatment, otherwise empirical, and seemingly inconsistent.

I would here take the opportunity of proposing the word "Acholia," as aptly implying that condition of the liver in which there is a suppression or deficiency of bile. It has its analogue in "Anuria."

In a state of "acholia," then, the function of a

viscus so large and important as the liver being interrupted, it seems quite natural to look out for some process of substitution; and it is evident that from the inner surface of the intestines alone could such substitution be *beneficially* afforded, for in no other place would bile, or any imperfect imitation of bile, be of any use; and *there* it is indispensable, not only for purposes of assimilation, but because its absence is, as already shown, a cause of irritation and mischief.

We know positively that in jaundice the constituents of bile display themselves in almost every tissue and fluid of the body; in the skin, in the urine, and even in the saliva. If so many tissues are thus capable, indiscriminately, of separating bile from the blood, how much more likely that the intestines should have this power, seeing that, after all, this, as already said, is the only place where it can be of any service, or rather, where it is absolutely needed; whereas, in all other positions, bile must be out of place, if not positively noxious. Seeing, moreover, as before pointed out, that the intestines are immediately in contact with the hypercholerized portal blood, hemmed in between them and the liver, should we not rather conclude that they would pre-eminently possess this vicarious faculty?

As for an objection that may possibly arise on the ground that the evacuations in jaundice are sometimes, or often, clay-coloured, I think that may

be disposed of at once, by considering that, where the supply of bile is already inadequate for the wants of the system, the whole would be taken up, in the course of the food through that long and tortuous tube so searchingly, as that no appreciable trace could possibly remain.

Every one is familiar with the seemingly contradictory effect induced by the exhibition of a purgative, relieving in one patient a constipation, checking a diarrhœa in another. The former result is intelligible enough; the latter has always been disposed of on the hypothesis—scarcely reconcilable with the pre-existent flux—of there being some irritating “matter” to be cleared away.

When the liver is presumably inactive—as when, for instance, the motions are colourless—a dose of calomel will produce stools at first, probably, olive or dark green, then greenish, and finally yellow. Here we have an absent secretion restored.

On the other hand, in bilious diarrhœa, where it has hitherto been taken for granted that the liver is not only acting, but acting too much, is it not somewhat strange that the same dose of calomel should be prescribed with a fair probability of a totally opposite effect, on an assumption of its efficacy to check the very same secretion when in excess? Is it likely that any medicine should possess such a specific control over one particular organ as to stimulate it to increased action when

inert, and to restrain that same action when it has become excessive? Should we not rather suspect herein some lurking fallacy?

But if we accept as probable a vicarious, though imperfect, elimination of bile from the intestines themselves in all cases where the hepatic function is temporarily interrupted, the action of mercurials, or of any other cholagogue, becomes intelligible enough. Whether the stools be colourless, as in some cases of jaundice, or "bilious," as in diarrhœa, we should then assume that, under both conditions alike, the liver was inactive; in the one case that little or no effort at a vicarious action had been made; in the other, that a free vicarious action had been established—and probably with partial relief. Consequently, in both cases we should administer calomel, not empirically, but with a rational view of restoring an absent and necessary secretion—hepatic bile.

We find that under whatever circumstances a sufficient dose of calomel, or other cholagogue, is administered—whether in cholera or acute dysentery, in both which diseases the motions are without colour—in bilious diarrhœa, or chronic dysentery, where they are more or less yellow—or in obstinate constipation, when they are absent altogether—in every case *one* result of the remedy will be the same; that is, the production of preliminary motions of a colour more or less intensely green, appearing some-

times, by reflected light, olive, or even black. It has been customary to attribute this colour to the peculiar effect of calomel, but it follows the administration of any purgative, though, of course, more observable in those that act most directly on the liver; and is even frequently seen where no medicine at all has been taken, especially in the intestinal irregularities of young children. Such motions, accordingly, I have always regarded as furnishing certain evidence of two facts; firstly, that the action of the liver has been lately suspended, wholly or in part; secondly, that that action has been anew and quite recently resumed.

This green or olive, more or less intense, is in fact the characteristic colour of bile that has been some time retained in the gall-bladder. During its retention there it becomes darker and thicker, a fact proved by appearances after death. In all cases of cholera, in which death has occurred *before the appearance of bile in the motions*, I have invariably found dark-coloured bile in the gall-bladder, sometimes distending it. And I find the same condition recorded by others, without, however, any notice of its relation to the other fact I have adduced—namely, an absence of bile from the motions during life.

A similar repletion of the gall-bladder with dark viscid bile is seen generally when no food has been taken for some time before death; and, under all

circumstances, cystic bile is darker and more concentrated than that found in the hepatic ducts.

During hepatic inaction, or a state of acholia, a portion of bile thus remains quiescent in the gall-bladder. But when the liver begins to act again, this cystic bile flows out simultaneously, to make way for the freshly-secreted bile. Thus, though dark-coloured bile is in fact *old* bile, its presence in the duodenum shows that the secretion of *new* has commenced. A green colour, more or less intense, in the motions therefore indicates, as I have said, *a renewal of hepatic action after temporary suspension.*

VI. With respect to *enteric*—terming it so in contradistinction to *hepatic*—bile, the question naturally next arises as to its proximate source. Is it furnished from the mucous surface of the intestines, from the glands with which they are studded, or from both? From a consideration of the facts before enumerated in support of a general enteric secretion of bile, but chiefly on the ground that both the mucous lining of the bowels and the glands are in immediate contact with portal blood, it may be inferred that a vicarious action is possible on the part of both.

In connexion, however, with this question, there is one suggestion that I would take this opportunity to offer, and which I trust will be found not without interest. By the researches of comparative anato-

mists the existence of a liver, in a rudimentary form, has been established, opening into the intestinal canal of animals very low in the scale. In these animals, therefore, we have a positive example of the very *condition*, the existence of which we have been labouring to establish; that is, a secretion of bile from the surface of the intestines—affording analogical proof, at least, of the physiological possibility of a similar secretion in man.

The rudimentary position, then, of the liver may be taken to be in the intestines themselves.

In the higher orders of living beings, therefore, may not the liver be physiologically regarded as a removal of these *rudimentary livers*, and their assemblage into one common organ? The distinct lobular structure of the liver supports this view.

On the other hand, instead of this aggregation into one mass, we may conversely imagine these lobuli, or *acini*, of the human liver interspersed throughout the intestines. Under such an arrangement the portal system—or, at least, the enteric portion—would of course disappear; this being merely an appanage, or *diverticulum*, of the venous system, rendered necessary by, and contingent on, the institution of a separate organ. Next reduce the number of these interspersed lobuli indefinitely, and we arrive at the rudimentary form of liver observable in the lower animals.

But further, the rudimentary position of the liver

being thus in the intestines, should we not, under the circumstances of its institution as a separate organ, be led to look for its *rudimentary* traces? *May not the various glands of the intestines, all or some of them, be in fact, in a subsidiary form, hepatic lobuli?* Modified by their position, probably, both in structure and in function, but still rudimentary remains of the same organ, and therefore capable also of furnishing a kindred secretion. For it must be always borne in mind that they are *in contact with the same "cholerized" blood.*

I will not here enter on an inquiry as to which class of glands are most eligible, from their structure, to be ranged amongst these hepatic subsidiaries, but content myself with referring to the general physiological fact that every secretive gland, including these intestinal glands and the individual *acini* of the liver, may be regarded as consisting of a *duct invested by a network of capillaries.* It is very likely, from their position, that the duodenal, or Brunner's, glands may possess this vicarious—or I would rather say *subsidiary*—function in a higher degree. But, at the same time, it is probable that, to some extent, it is shared by them all—at least vicariously; since they are all formed on the common type of a *duct invested by capillaries*; are in contact with cholerized blood; and occupy the rudimentary position of the liver.

The following passage from Abercrombie's "Dis-

eases of the Stomach" will be found worthy of attention :—

"It is probable that the bile may be increased in quantity; but at the same time it must be admitted that our prevailing notions on the subject are rather hypothetical than founded on facts. When the motions become of a dull white or ash colour, we judge with tolerable precision of the deficiency of bile; but I am not aware of any test by which we can judge with precision of its redundancy; and I must confess my suspicion, that the term "bilious" stools is often applied, in a very vague manner, to evacuations which merely consist of thin, feculent matter mixed with mucus from the intestinal membrane. On this subject I find a late intelligent writer (Mr. Tytler), on the diseases of India, expressing himself in the following manner, after alluding to the doctrine of several systematic writers in regard to *bilious diarrhœa, arising from increased secretion of bile*: 'Not a single fact is produced by either of these authors in support of their opinion, and it seems to rest merely on the popular notion that the colour of the feces is derived from the bile; but this doctrine seems rather to be taken for granted than proved.'"

I was much pleased, in coming on the above-quoted passage, to find my own doubts as to the nature of "bilious" diarrhœa thus anticipated, and by such high authority. At the same time it seems

somewhat strange that it should never have struck either of these accurate observers to inquire—*If the yellow matter in bilious stools did not come from the liver, whence it did come.*

As the pancreas furnishes a part of the portal circulation, and is at the same time a secreting organ, I may be expected to express an opinion as to whether it shares in the subsidiary hepatic action I have attributed to the intestines. From a consideration of the common type on which all secreting glands are formed, and from observance of such an apparently universal law of vicarious action, I have no doubt that, physiologically, the pancreas might secrete a factitious bile. But I think it unlikely that this faculty is ever called into operation, excepting perhaps under circumstances of special urgency, and only when the whole mass of the circulation has become “hyper-cholericized,” or surcharged with biliary constituents; firstly, because it is not, like the enteric glands, in contact with cholericized blood, or blood that has already served the purposes of assimilation; secondly, because it does not, like them, occupy the rudimentary position of the liver; and, thirdly, because it has a manifestly distinct and special function.

To this subject of an enteric secretion of bile I shall have hereafter occasion to revert in treating of diarrhœa, dysentery, and cholera. I shall therefore limit myself here to such illustrations as occur

to me of its apparent existence in some other diseases that are necessarily without the scope of this work.

I would accordingly refer to a few well-known pathological conditions, under which, in consequence of structural degeneration, or from other causes, the proper function of the liver may be assumed to have been almost annihilated, and wherein, nevertheless, the assimilative process by the bowels seems to have been carried on with comparative success; wherein, moreover, the bile furnished seems to have been more than adequate for the wants of the system, inasmuch as the *egesta* have been tinged with its residuum; results totally inexplicable except on the assumption of its being a vicarious secretion.

VII. The following case, by Dr. Alison, published in the *Edinburgh Medical and Surgical Journal*, 1835, page 287, is interesting in many pathological points, but particularly as respects the *post-mortem* appearances of the liver and intestines. It will be observed that the hepatic function had evidently been suspended for some time before death, and that nevertheless a greenish bile was found in the intestines; and, what is highly significant, *more in the lower than in the upper portion*.

“ Peter Schread, aged about twenty-five, a German sailor, was admitted into the Clinical

Ward, 26th February, 1826, in a state of complete delirium, with tendency to violence, but alternating with drowsiness. His skin and *tunica conjunctiva* of the eyes were of a bright yellow colour; he had no tenderness of abdomen; his pulse was 60, of irregular frequency; tongue moist; extremities rather cold; he had occasional *singultus*; he passed a copious *bilious* stool, and also urine, in bed soon after admission.

“His companion reported that he had a severe attack of flux, in Java, in the summer previous; that he had been in good health at Antwerp from September till December, but that since 1st January, when he arrived at Leith, he had complained often of pain and heat in the abdomen, chiefly towards the right side, with thirst and chilliness; that eight days before admission he had become jaundiced, and two days before admission had become delirious.

“His head was shaved, bathed, and blistered, and he had one dose of calomel and several of tartar-emetic (the only medicines that could be got down), which produced copious *bilious* stools, all passed in bed; but the delirium passed into complete coma, with dilated pupils and stertor; his pulse rose to 120, and became feeble; some purplish spots appeared on the skin, and he died on the evening of the 28th, ten days after the appearance of jaundice.

“ ‘The following account of the dissection was drawn up by Dr. C. Henry, of Manchester, who then assisted me as one of the clinical clerks :—

“ ‘The skin and subjacent cellular tissue were universally of a bright yellow colour. This tinge extended also to the pericranium, and to both surfaces of the *dura mater*, which was rather more vascular than natural. The other membranes of the brain were dry and glistening. The bloody points were somewhat more numerous than usual. There was very slight distension of the left lateral ventricle, the contained serum not exceeding half a drachm. That found in the right was still less considerable, and there was hardly any at the base of the brain, which appeared somewhat vascular. The consistency of the cerebral structure was perfectly healthy.

“ ‘The surfaces and central points of the cartilages of the ribs were tinged with bile, as were also the peritoneum and pleura. The liver, when incised, appeared of a light yellow colour; it was smaller than natural, its structure dense and resisting compression, but in mass it was remarkably large and flexible. The calibre of the cystic duct seemed to be in part obliterated; but the hepatic and common biliary ducts were quite pervious. Their mucous membrane was unnaturally white. The gall-bladder contained a greenish, viscous, semi-fluid matter. The spleen was somewhat firmer than natural. The

pancreas was healthy. The contents of the intestinal canal were tinged, though slightly, by a greenish bile; *those of the lower part of the ileum less than of the larger intestines*. There was no vascularity of their lining membrane, but that of the great intestines appeared somewhat thicker than usual. The mucous coat of the bladder had acquired a deep yellow tinge, and contained urine of similar appearance.'"—*Edin. Med. and Surg. Journal*, 1835, p. 287.

I next offer a case of infantile bowel complaint, by Dr. Scott, published in the *Edinburgh Medical and Surgical Journal*, 1835, p. 102. It will be observed that the bowels were found to contain a greenish matter, which could not reasonably be supposed to have come from the liver or gall-bladder. Similar greenish discharges prevailed during life, and until death occurred.

“CASE.—*Infantile Bowel Complaint. Dejections uniformly green or yellow. Gall-bladder distended with tarry bile, so viscid that it could not be forced through the duct.*

“A. B., a boy, ten months old, while at the breast, had been twice attacked with bowel complaint of five or six days' continuance. The dejections were generally of a green colour, sometimes consisting of mucus tinged with blood. This yielded to minute doses of calomel, Dover's powder, and castor-oil. In other respects the boy had been

thriving. When apparently in good health, he was weaned, which was done without difficulty, as he took his food well from the first. About a fortnight afterwards I was requested to visit him, and found that, three days before,, the bowel complaint, *with green dejections*, had again appeared. There had been no vomiting, no heat of skin, and he took his food well. For the next three days the dejections were frequent, consisting either of mucus slightly tinged with blood, or of green matter, resembling chopped spinach. There was apparently little pain at any time, nor did pressure seem to produce uneasiness. The abdomen continued soft. The disease went on, the child becoming gradually weaker; at times the purging intermitted for ten or twelve hours, and the dejections *sometimes changed to a yellowish colour*, but there was never any proper feculent matter. Towards the termination there was at times a remarkable tossing of the head, with great restlessness. For eight or ten hours he would refuse food, and then take it voraciously. The skin was cool throughout the progress of the disease. There was vomiting and little thirst till the last stage. Various remedies were tried. Laudanum, in a dose of two drops, produced a deep sleep for ten hours. One grain of the compound powder of ipecacuanha produced nearly the same effect. The infant died on the twentieth day, and was examined the next morning.

“The liver, when cut into, was of a healthy appearance—perhaps of a redder colour and more vascular than natural; *gall-bladder excessively distended with a dark-coloured bile of very viscid, tarry nature, which could not be pressed by any force through the duct into the duodenum, though the passage was unobstructed.* The stomach was healthy, as also the track of the intestinal canal, till we arrived at the colon, the mucous membrane of which, through the whole extent, was much thickened, of a consistence firmer than usual, and everywhere spotted with small red specks, as if red sand had been scattered over the surface. There was some fluid between the arachnoid and pia mater.”

In the course of his remarks on this case Dr. Scott observes—“In systematic works we are told that the green discharges indicate a superabundance of bile. It may be doubted how far this is correct. In the present case the discharges were generally green, and the gall-bladder was gorged with thick bile, almost of the consistence of tar; a very common occurrence in the examination of patients who die of dysentery.”

I have next selected a case by Dr. Bright, which I find quoted by Dr. Budd, and published in the first volume of “Guy’s Hospital Reports.” As in the above cases, I have ventured to italicise those portions having special reference to the subject.

“ CASE.—*Jaundice. Inactivity. At the end of a fortnight, vomiting and delirium, soon followed by coma and death. Liver unusually small, and of a brightish yellow colour, marked with purple or deep brown. Gall-bladder small and collapsed. No trace of bile in the common or hepatic duct. The quantity of serum within the skull unusually small. No structural lesion of the brain.*

“ Keatrina Pfifrein, aged eighteen, was admitted into the Clinieal Ward, January 11th, 1832, labouring under ieterus. She was an assistant to a German broom-maker, and was unable to speak any English. The skin was of a brilliant yellow ; and the cheeks, which were flushed, were of the colour of a very ripe aprieot. She appeared exhausted ; and though she answered questions pretty readily, we were eautioned by a woman who brought her that her replies were inecorreect. Pulse 120, very small and weak ; feet and body very cold. We learned that when she came to London, about a fortnight ago, she had been already unwell about a fortnight ; and her skin had a decidedly yellow tinge, which had daily inereased, attended with an inaetivity amounting almost to torpor ; so that, when removed from her bed, and placed by the fire, which was all she could bear of late, she sat eonstantly in a kind of doze. We were told that her bowels had been relaxed, without much abdominal pain ; and she had not

suffered from sickness. She had complained but little of headache ; tongue moist, and slightly furred ; the papillæ prominent.

“ She was ordered a moderate dose of hyd. e. eretâ three times a day, and light nourishment and warmth ; and should it not prove, as had been stated, that her bowels were relaxed, she was to take some coloeynth pills at night.

“ Jan. 12th.—She was sick yesterday evening, vomiting a good deal ; she lay in a perfectly torpid state the whole night, apparently suffering no pain ; but towards the morning became delirious, so that it was with difficulty she could be restrained in her bed. At the time of the visit she was very restless, and seemed to suffer pain, but was unable to answer any questions ; indeed, except that she swallowed what was given to her, she seemed scarcely conscious ; and it was quite uncertain whether pressure on the abdomen gave her any pain. The pupils were dilated ; the bowels had not been open, although she had taken two compound coloeynth pills ; pulse 106, thrilling, and compressible ; tongue moist and clean.

“ She was ordered two grains of calomel every two hours, and the ammonia julep every four hours ; besides wine, if she became more depressed. Her head was shaved, and a blister applied over the liver ; mustard poultices to the feet ; and camphor mixture was to be given freely, in case the delirium

should return; injections were to be repeated till the bowels acted freely.

“During the night the purging injections, with colocyath and castor oil, were administered three times; she lay completely comatose the whole night; the pulse sometimes at 140, and extremely weak, when not raised by stimulants.

“No dejection having been passed at ten o’clock in the morning, another colocyath injection was administered, which produced copious, rather dark, unhealthy, feculent motions, mixed with some sanguinolent fluid; and there was likewise an appearance like pus. The blister discharged very abundantly; the urine was passed involuntarily, and in considerable quantity; mouth and lips covered with sordes; pulse 120, weak.

“A blister to the crown of the head; the calomel to be repeated.

“She continued to sink during the day, and died at ten o’clock in the evening.

“*Sectio Cadaveris*.—The whole external surface of a deep yellow colour; the adipose matter was also yellow, as were the cartilages of the ribs.

“The lungs were healthy, but the posterior portions gorged with blood, probably the result of her having been lying for two days on the back. The pleura of the left lung of a slight yellow tinge; the heart healthy.

“The whole of the abdominal viscera, when first

exposed to view, were remarkably tinged with bile; the stomach of a vivid yellow; the intestines looked green; the liver was unusually small, and for the most part of a brightish yellow colour, with portions marked with purple or deep brown; and, in parts, a finely spotted appearance was yielded by the acini. On cutting into the liver, the same yellow colour, with fine dark spots, pervaded it. The gall-bladder was very small and collapsed, and contained less than a teaspoonful of thick ropy mucus of a bright green colour. The cystic duct appeared to be quite contracted; so that neither could a fine probe, nor the point of a scissor, be carried along more than two-thirds of its length upwards; nor could the tenacious mucus of the gall-bladder be forced down it. However, there was no appearance of thickening, or of morbid deposit, either within or around the duct, which, when laid open with the scalpel, presented the corrugated valvular appearance peculiar to that part of the duct. The lower part of the cystic duct, as well as the whole of the hepatic duct and the common duct, quite into the duodenum, were pervious, and not at all thickened or diminished from the natural calibre. There was no trace of bile in either of the ducts; and following the hepatic ducts quite into the substance of the liver, no bile was detected; but on squeezing the liver, the small secondary and tertiary subdivisions of the

duets were seen filled with thick tenacious mucus, of an exceedingly faint lemon-yellow colour.

“The mucous membrane of the alimentary canal was perfectly healthy, but the contents were very unnatural; in some parts of the ileum and jejunum there was yellow mucus; in others, an olive-green mucous excrement; and in the colon, a drab-coloured and grey mass, characteristic of that which usually composes the fæces of jaundiced patients.

“The spleen soft; pancreas healthy. Kidneys tinged throughout with bile. Bladder somewhat distended, rising to view above the pubis, and containing probably a pint of clear yellow urine.

“The thoracic duet quite empty. The arteries deeply tinged with bile.

“The dura mater was of a brilliant yellow colour; the arachnoid not vascular, and quite untinged with bile; there was no unnatural effusion of serum beneath it; but the small quantity which collected in a few of the sulci was very slightly tinged with yellow, as were the few drops which collected in the base of the skull, when the brain was removed. When slices of the brain were taken horizontally, a moderate number of cut vessels were seen; many of the small points of blood gave a stain of beautifully yellow bile around them; and some points gave out the yellow serum, without any blood appearing. The ventricles contained an unusually small quantity of serum; and that was not tinged

with bile. The quantity of serum throughout the whole brain was decidedly deficient. There was no structural lesion nor irregularity in the brain."

In the above case the motions are characterized as "rather dark, unhealthy, feculent;" and though it could be wished that they had been more distinctly described, especially with respect to colour, it is quite evident that they were not of the white or clay colour generally met with in jaundice. But the *post-mortem* appearances are very interesting, and accord with what I have often seen myself. Thus, in the upper part of the digestive canal was *some* yellowish matter; in other parts an olive-green excrement; whilst below, in the colon, was a "drab-coloured and grey mass," which, we may fairly conclude, had once itself possessed a yellowish or olive-green tinge, owing to a colouring matter which must have been re-absorbed, after the manner of true bile, in its course downwards. But from the condition of the liver, gall-bladder, and duets, it is evident that this colouring matter had not its source in the liver.

The three cases above quoted I have selected as illustrative of an enteric secretion of bile; but I could have cited many parallel instances within my own knowledge, and others abound in medical works. As premised, however, in the Introduction, I shall consistently refrain from quoting facts that rest within my own experience only, so as to avoid

the least imputation of their having been observed under the influence of a foregone conclusion, or of preconceived opinion. The few illustrative cases I have selected will be invested, therefore, with the greater weight, as having been recorded by their authors without the slightest reference to the views advanced by myself, and which views I suppose them calculated to support. I shall only add the following brief sketch of a case, jointly observed by Surgeon Starkey, then of H.M. 97th regiment, and myself.

We inspected at Kirwee, in 1858, the body of an infant of a month old, in which we could discover no trace of a gall-bladder, nor of a *ductus communis*, or other communication with the duodenum. The liver was small, but healthy. The child had been apparently well, and even robust, with a good appetite, till within ten or twelve days of its death. During this time it had suffered from diarrhœa, not very profuse, with fits of violent pain, gradually increasing in frequency and severity. The evacuations had been uniformly of a bilious aspect—pale yellow, never green or dark. Jaundice had supervened two days only before death. In this case the evidence of an enteric supply of *quasi* bile seems most positive and unmistakable.

CHAPTER II.

ON THE NATURE AND PROPERTIES OF BILE.

Urere bilis jecur.—HOR.

Bilem, sæpe jecum, vestri movère tumultus.—HOR.

A Deficiency of Bile a Cause of Irritation and Disease, not an Excess of it—Antiquity of an opposite Opinion—Modern Opinion limited to Mechanical Obstruction as a Cause of Disease—Spasm of Ducts untenable—On the Excessive Use of Purgatives—Abscess a consequence—Cases from Annesley—Distinction between an Excretion and a Secretion—Secretions cannot become per se injurious—No General Analogy between Urine and Bile—Elective Property of a Secretion proper—Suspension of an Excretion more fatal than of a Secretion—Consequences of a Suspension of Bile—Constipation—Diarrhœa—Dysentery—Cholera—Argument against Acrimony of Bile from Pathology of Dysentery—Pathology of all Diarrhœæ analogous to that of Dysentery—With these views our Treatment rendered “rational”—Anomalous Effects of Calomel explained—Abercrombie’s Doubts quoted—Arguments from Nature of Jaundice—Icterus Cholicus and Icterus Choloides—A Condition of Jaundice not incompatible with Health.

THE prevailing notion regarding the nature of bile appears to be, not that it is a secretion in itself bland and soothing, or even gently stimulating, but rather that it is, at least on all occasions of variation from health, an acrimonious fluid, whose presence within the bowels is sufficient to account for

every morbid indication, and which is therefore to be, by all means in our power, expelled for the present, and restrained for the future. I think I am further warranted in stating that a pretty general opinion prevails, in tropical countries particularly, that a redundaney of so-called aerid bile is a frequent cause of various allied diseases, amongst which may be specially enumerated diarrhœa, dysentery, hepatitis, and cholera,—at least in its sporadic form.

I believe that this idea, so far from being correct, is, in most instances at least, the reverse of the truth; and I propose to show, in the first place, that a temporary suspension of the function of the liver, with a consequent absence of bile, is a far more common defect, and at the same time an inevitable cause of irritation in the intestines, which nothing but a reproduction of that secretion can assuage or remedy; secondly, that this irritation shows itself in various forms, affecting under different conditions different parts of the intestinal tube; and, thirdly, that these varying forms of irritation do in themselves respectively constitute specific diseases, as dysentery, cholera, and diarrhœa.

That constipation, on the other hand, is one of the ordinary results of a *diminished* secretion of bile, is an opinion so general, that I shall probably not be expected to demonstrate its correctness par-

ticularly. In what way, however, various degrees of deficiency in the amount of bile should thus induce, sometimes constipation, sometimes diarrhœa—with its supposed congeners, dysentery and cholera—affords matter for interesting, and in its practical application most important, inquiry.

In the whole history of medicine there is no opinion that has been more unanimously adopted, from the time of Hippocrates downwards, than this respecting the nature of bile; denounced on all hands as a noxious matter, hostile not only to bodily health, but incompatible as well with a state of moral well-being—bad of any colour, but worse when black. The etymology of such words as *choler*, *choleric*, and *melancholy* testifies to this familiar association of ideas in comparatively modern times; whilst the works of poets, Greek and Latin, abound with allusions, all referring to the same belief. Such long-established popular errors are rarely without some foundation in truth. The ancients accordingly were well aware that some strong relation here subsisted between mind and matter, between the biliary function and the moral nature; though, in accordance perhaps with a humoral pathology, effects were referred always to excess or vitiation, never to a diminution or absence, of bile.

A deficiency of bile *in the intestines* is now, however, acknowledged to be an occasional accompaniment of impaired assimilation, and also of certain

forms of diarrhœa ; but I am not aware that any medical writer has distinctly assigned an insufficient action of the liver as a frequent cause of this latter class of diseases. Its operation has indeed been referred to, but always as attendant and subordinate, never as the chief and sole cause ; whilst a closure or obstruction of the common or cystic ducts has been assumed as proximate and essential.

Even in cholera, because generally after death bile has been found in the gall-bladder, the conclusion has been too hastily adopted, that some impediment to its egress must have existed during life ; though in every such instance the struggle has been prolonged, and there has been an evident and partially successful effort at reaction. For in the worst and most rapidly fatal cases, where there has been no such effort, the gall-bladder *is invariably found empty*.

That the cystic and common ducts possess a power of contractility I have not the least doubt. I have always held that a *peristaltic* action on their part was probably conducive to the extrusion of biliary calculi, and even of bile. Doubtless, too, the common duct may, from continued disuse, become shrivelled and permanently contracted, as is sometimes seen in cases of cirrhosis and other hepatic degenerations. But the notion of *persistent* spasm is not only purely gratuitous, but opposed to its histological structure, and to its function as an

effluent canal. Its temporary closure by inflammation must also be received as a rare and exceptional occurrence, not to be taken into account in the pathology of either diarrhoea, dysentery, or cholera; not only because seldom observed after death, but also because such inflammation is necessarily of a suppurative or ulcerative character, very rarely adhesive or *plastic*.

Reverting to this traditional belief in the hostile character of bile, not unfamiliar still are the occasions on which physician and patient concur in a notion of its redundancy. To expel it, therefore, calomel, or some other cholagogue,—and all purgatives are more or less *cholagogues*,—is administered, with the result of a copious evacuation of “offensive stuff,” and sensible relief to the feelings of the patient, who felicitates himself on being quit of such a quantity of “horrid” bile, which (as is somewhat loosely concluded) must have been accumulated *within the bowels*. Now in such a case the treatment, as the result would seem to indicate, has been in all likelihood judicious enough; it is the pathological premiss alone that is open to question.

Happy for the patient if, at this stage of the treatment, his medical adviser should pause, content to await—after a subsidence of the temporary disturbance in a natural and healthy action of the bowels—the *secondary* effect of his remedy. But

this is by no means always the case. There are not wanting those, in tropieal elimates particularly, who regard bile in the light of a natural enemy, to be ousted at any cost—*vi et armis*—from his stronghold. With such the appearance of a decidedly bilious motion, in place of being accepted as satisfactory evidence of a resumption of the hepatic function, is too often regarded as the signal for a renewed attack; a proof that not enough has been done, that the enemy has only partially evacuated, and that there is more “villanous stuff” to come away. Another purgative therefore, stronger perhaps than the first, is administered, with of course a similar, but more striking effect. Another “*offensive, bilious-looking motion*” is recorded, and preparations are made for a fresh struggle with this indomitable enemy, who appears, with each successive defeat, to gain rather than lose strength. A third and fourth dose are accordingly prescribed, the previous notion of a redundancy of bile being about this time strengthened by certain *ominous twinges in the right hypochondrium*. To the more sagacious observer, however, these should convey a very different impression. He would conclude that mischief, probably irreparable, was done at last; that that grand and long-suffering organ, the liver, had been stimulated beyond its powers, marvellous though they be, of resistance and repair; that goaded daily to renewed exertion, it had continued

to pour forth fresh floods of its beneficent secretion, thus for a while relieving the system and itself of an implacable tormentor. But such extraordinary efforts cannot be long sustained without the gravest consequences. The great portal system being more and more drained of the elements needed for the extraction of bile, the task of the liver becomes necessarily more and more arduous. To aid in this work of mingled difficulty and danger, let us imagine the sentient blood coursing more swiftly—each day more swiftly, hurriedly, tumultuously. Then follow, in inevitable and fatal sequence, obstruction, congestion, inflammation,—**ABSCESS !**

It seems not a little extraordinary that, amongst the causes of hepatic inflammation and hepatic abscess, this excessive use of purgatives—limited, I believe, to tropical climates—should have been overlooked. It may be hoped that the practice is already declining : from my own observation, I should say so decidedly. But at the same time it is certain that the published opinions of those who advocated and pursued such a style of treatment did lately, if they do not still, constitute authorized text-books in India. Every regimental hospital there, and, I believe, the civil as well, is furnished with a few books ; and prominently amongst them two ponderous tomes, well known as “Annesley’s Diseases of India.” Before the

extinction of the old Company,* every medical officer, on landing, was presented with these last, and also with a coloured porter to carry them behind him wherever he went. Far be it from me to imply that they were unworthy of so much regard. In the days when they were issued, they comprised a well-written and valuable fund of knowledge and original observation, and even now are well worthy of attentive perusal. But modern notions would scarcely approve of the vigorous measures therein inculcated—less adapted too, perhaps, to modern constitutions. *Tempora mutantur, et nos mutamur in illis.*

* I cannot forego the opportunity of contrasting the Indian medical service as it is now with what it was but a very few years ago, and before the “extinction of the old Company,” and congratulating my brethren, the juniors especially, on the change. For nearly nineteen years I was myself an assistant-surgeon, with the rank and pay of a lieutenant. Now the Indian medical officer attains, as a matter of right, the rank and pay of captain *in six years*! Promotion in the upper grades has become indeed lamentably slow; but I think this attributable, not so much to any inherent defect in the constitution of the service, as to the pernicious working of our Annuity Funds, whereby promotion is strictly limited to the number of annuities yearly available.

Does it not seem almost incredible that, until our happy transference to “Her Majesty’s” rule, all the medical appointments were virtually in the gift of an adjutant-general, who, in his complete ignorance of what constituted professional merit, might well be pardoned for favouring his personal friends? whereas the very life of such a service depends on such selection being vested in the hands of an administrative head who, both by his professional training and personal knowledge of individuals, is able to recognise and encourage merit in the lower grades. As I thus recall it to mind, the Indian service was one to which no young man, entertaining a proper feeling of self-respect, could belong without a sense of humiliation, not to say degradation; and I rejoice that I have lived to see it raised to a position of comparative dignity and independence.

Annesley, in his own experience, met with abscess as such a constant attendant on dysentery, that he arrived at the conclusion that the former was the immediate cause of the latter; that, in fact, "abscess was dysentery in disguise." I would appeal, however, to the general experience of the present day to declare whether abscess, instead of being thus commonly associated with dysentery, has not become, even in fatal cases, comparatively rare. Dr. Abercrombie, in his "Diseases of the Stomach," page 250, has the following passage:—"I have never seen the liver affected in the dysentery of this country, except in one or two chronic cases, to be afterwards mentioned. It seems to be of more frequent occurrence in Ireland, and still more in India; but in the dysentery which was so fatal to the troops at Rangoon, in the Burmese war, Mr. Waddel states that he did not find disease of the liver *in any one of his dissections*. I am also informed by Dr. Knox, of this city, that he had opportunities of examining the bodies in sixty-four cases of chronic dysentery from India, Ceylon, and the coast of Africa, and that he found the liver diseased in two only of all this number."

Mr. Twining, in his "Diseases of Bengal," gives similar evidence of the comparative rarity of disease of the liver in those who have died of dysentery.

To avoid any suspicion of a prejudiced or *ex parte* statement of my own, I subjoin a description of

Annesley's treatment, as given by Abererombie. At page 284 of his "Diseases of the Stomach," he says: "Mr. Annesley begins the treatment of dysentery in a robust patient with free general and topical bleeding, and a large dose of calomel, combined with opium or Dover's powder: in debilitated habits topical bleeding is employed. These are followed, at the distance of a few hours, by a purgative of castor-oil, or jalap and cream of tartar, and a purgative injection. The calomel and opium are also repeated after a few hours' interval; and this treatment is assisted by warm fomentations, warm baths, and anodyne injections in very small bulk. These remedies are afterwards repeated according to circumstances, with blistering on the abdomen, if necessary; *and a purgative is generally given every morning.*"

I cannot help entertaining a strong conviction that a large proportion of the abscesses of former days were owing simply to over-purgation; not so much to the dose of calomel overnight, as to the cathartic repeated morning after morning, and *as long as anything "offensive" came away.* I had intended to extract some of the cases recorded in Annesley's great work, as illustrative of this overstimulation of the liver, and consequent abscess of that organ; but I am reluctant to give the space they would occupy, and am also apprehensive of wearying the reader, who has only cursorily to turn

over the pages of the work referred to to be satisfied of the correctness of the account given above. It has not been without much hesitation that I have ventured thus to call in question the practice of a deservedly eminent man, and have ever had before my eyes Quintilian's caution : " Modestè tamen, et circumspecto judicio, de *tanto viro* pronunciandum est, ne, quod plerisque accidit, damnet quod non intelligunt."

Proceeding with our inquiry into the nature and sensible properties of bile, I will now endeavour to adduce in detail some arguments opposed to the circumstance of its ever becoming an acrid or irritant fluid.

An *excretion*—by which I understand *a product to be rejected* from the body—must be, from its very nature and purpose, liable to great variety of composition; and its purpose being the removal of superfluous matters, when these are of a more than usually noxious or irritating character, the excretion must necessarily partake of such properties, and become itself noxious and irritating. Both the urine and perspiration are thus liable to striking and sensible variations.

The word *secretion*, on the contrary, should be restricted to glandular products—as milk, semen, saliva, and mucus—that are destined to some beneficial use in the economy.

From this *beneficial use*, characteristic of every

secretion, we might expect, *à priori*, that its composition should not be subject to *material* variation, or to such as would altogether destroy *its beneficial use*.

And such would appear to be the case. I hold it to be a general law, that when the structure of a secreting organ reaches such a stage of disorganization, or deterioration, as that thereby its secretion would altogether lose its beneficial use, or become positively noxious, *that secretion stops*.

Milk, semen, saliva, mucus—these secretions may become weaker, thinner, denser, or more viscid, less adapted in various ways for their respective offices; but never, I believe, *altogether* destitute of their proper beneficial use in the economy, or directly injurious.

When the whole of a secreting organ, as already said, becomes unfit for the production of a *beneficial* secretion, its function is suspended. When, however, part only is involved, the function of that part only is suspended, the remainder continuing to furnish a *beneficial* product.

Such then being the specific and essential character of those secretions that we are able to examine, as milk and semen, we are furnished with one strong ground for inference that the secretions of the liver and pancreas are subject to the same general law; that is, that bile and the pancreatic juice never become so much changed as to lose

altogether their beneficial properties, much less to become noxious or irritant.

For they are perfectly analogous to the other secretions ; being, *firstly*, products of glands formed on the common type of “a duct invested by capillaries ;” and, *secondly*, destined to ulterior and beneficial use.

On the other hand, nothing can be more likely to lead us into error than to establish conclusions founded on a fancied analogy between the liver and kidneys. We know a good deal about the variations in the functions of the latter ; little in this respect, except by inference, of the former. Hence the temptation to avail ourselves of an assumed analogy. But a little consideration must lead to a conviction of the absence of all essential points of comparison. The very objects of the hepatic and renal functions are opposed to each other. The office of the liver is to provide a fluid for a certain definite use in the economy, and which, therefore, must possess certain definite properties. The proximate chemical constitution of this fluid must not materially alter ; it may be thicker, thinner, changing in hue from bright yellow to dark olive ; but, on analysis, it must, to be of use as bile, yield, varying only in relative proportions, the same proximate elements. In this respect it is properly analogous to milk, the saliva, the seminal fluid, and other correctly so-called *secretions*.

The purpose of the renal function is diametrically opposite. Instead of having to provide a fluid of a certain definite composition, it matters not (so to speak) what its composition be, so that certain noxious elements present in the blood are removed. Whatever these may be—sugar, lithic acid, oxalates, urea, essential oils, colouring matters, salts, bile itself—all are indiscriminately apprehended by this universal gland, which seems to have in its working no power whatever of selection.

But a distinctive feature of the liver, and of every SECRETING gland, is that it does possess this elective faculty; extracting, from a heterogeneous and ever-varying fluid mass, just those elements that are wanted, and as consistently rejecting all others. For it is evident that all these extraneous matters alluded to pass through the liver precisely as they do through the kidneys.

The urine thus consisting of matters foreign to the blood, and whose presence therein must consequently be hurtful to the body, we might augur that any interruption or suspension of its *excretion* by the kidneys would be followed by serious results. And in point of fact we know that its total suspension for even a short time is generally fatal to life.

On the other hand, the office of the liver being to eliminate certain elements from the blood, which almost immediately again have to be restored into the same blood, we should similarly expect that an

interruption of this process would not be attended with consequences so grave. And this too is consistent, as we know, with fact.

The addition of bile, however, being necessary for a due performance of the assimilative process and the provision of healthy chyle, it follows that every diminution of the needful amount must be followed by a corresponding defect in the work of digestion, of which we are occasionally furnished with striking indications in the character of the *excreta*.

Thus, when there is a minor deficiency in the supply of bile, we have constipation; when this has lasted some time, the bowels, irritated by continued contact with "hyper-cholericized" blood, pour out mucus, converting the previous obstipation into a diarrhoea; an operation explained in the preceding chapter.

In some other condition of the system, dependent perhaps on malarious influence, the function of the liver appears to be altogether suspended; a severer form of enteric irritation ensues, succeeded by ulcerative inflammation, and constituting the commonest type of "acute dysentery."

During these temporary suspensions of the liver's function it may be assumed that the kidneys, whose office is of so versatile and accommodating a nature, act in a subsidiary way, removing from the blood (in a form proximate or remote, or in both at once) those elements that the liver has failed to convert

into bile, and which therefore must tend to accumulate in the blood.

But whilst the function of the liver is thus temporarily suspended, suppose, from any cause, that of the kidneys simultaneously arrested.

We know of only one complete example of this terrible complication, and the result is—CHOLERA.

If bile were ever this acrid and irritative fluid that it is supposed sometimes to be, we should expect that this quality would be first exhibited in the biliary ducts and gall-bladder; next in the duodenum; then in the jejunum and ileum; and so on through the colon; and lastly, and in the least degree, in the rectum. Is this the case? In dysentery, for instance,—in which disease the inflammation, proceeding to ulceration and sloughing, is uniformly and confidently attributed to the irritation of acrid bile,—what do we observe at the onset of the attack? Passing pains in the abdomen; not constant or severe, as in colic or peritonitis; and, on palpation, clearly referrible to the colon, and especially to its terminal flexure; but in the rectum, on going to stool, intolerable anguish, and a feeling as if there were passing molten lead. But no bile *there*—not a trace.

And to what conclusion are we led by the morbid pathology of the same disease? Does this belie the symptoms observed during life? Do we find traces of inflammation commencing at the liver's outlet—

in the gall-bladder, where rests, and in the duets, where constantly streams, the virgin bile—next in the duodenum, and so on less and less as we descend? Need any pathologist be told that the reverse of all this is the fact; that the traces of severest inflammation, the largest patches of ulceration, are to be found in the lower bowel, diminishing as we *ascend*, till in the small intestines the existence of ulceration at all becomes rare, and in the duodenum and hepatic duets (as far as I have been able to ascertain) unknown? Instead, then, of there being anything either in the symptoms or pathology of dysentery to warrant the supposition of bile being an acrid fluid, and the cause of the disease, all we observe tends to establish a directly opposite conclusion.

And what affords the first sign of amendment and relief in this distressing disease? A subsidence of pain and tenesmus, immediately succeeded by the *first bilious stool*.

In what other way can we explain these familiar phenomena, except on the hypothesis that *not bile, but its absence, is a proximate cause of inflammation; and, conversely, that its restoration is the natural, indispensable, and only means of perfect cure?*

I have, in the first chapter, indicated reasons for doubting that the yellow colouring matter of the evacuations in so-called bilious diarrhœa is bile, the product of the liver; and also for suspecting

that this may in truth be the effect of a vicarious secretion by the intestines themselves—enteric bile. On these hypotheses the pathology would be analogous to that of acute dysentery, as above briefly enunciated; and such diarrhœæ would be caused not by “a redundancy of acrid and irritant bile,” as is popularly supposed, but by an actual insufficiency of this natural secretion, which the intestines have, with but partial success, endeavoured to supply. In dysentery, too, sooner or later, if relief in the form of *hepatic* bile come not before, they will similarly commence to act; this action constituting another stage of that disorder which, when prolonged, acquires the name of “chronic dysentery,” being in fact but a form of diarrhœa, the sequela of the primitive disease.

I cannot help here pointing out, as I have elsewhere, that the common and most successful treatment of these and other kindred diseases is rendered at once uniform, “rational,” and perfectly intelligible on the hypothesis here maintained. For whether purgatives, or mercurials, or ipecacuanha, or tartar emetic be employed, the *modus operandi* is the same; they all act as *cholagogues*, and until this cholagogue effect is induced, and the liver, so to speak, unlocked, there can be no real amendment, and no cure.

Johnson, Abererombie, and Annesley—these accomplished physicians in succession testified to a

very remarkable peculiarity in the action of mercury, which universal experience has since confirmed. It is this : that small doses of calomel prove irritant, and increase the looseness in diarrhœa and dysentery ; whereas large ones prove sedative, and often arrest the purging at once. An analogous result will have been noted by those few who have treated cholera by calomel *alone* in large doses. But I am not aware that any satisfactory explanation has been offered of this seeming anomaly. On the assumption, however, that the liver's function is suspended, or inadequate, the *modus operandi* becomes perspicuous enough. When small doses of calomel are taken, they suffice to irritate more the already irritated bowels, but not to open the obstructed sluices of the liver ; thus the purging is increased. But when a single large dose is administered, the train of phenomena, before more fully depicted, is awakened. The bile wells forth freely ; the hyper-cholericized portal blood, thus purified, loses its irritant quality ; and the bowels, simultaneously freed from two sources of irritation, hyper-cholericized blood and the want of bile, subside into the gentler action of health.

This apparent inconsistency in the use of calomel seems particularly to have struck Dr. Abererombie, and he more than once recurs to the subject, evidently regarding such treatment, not referrible to any "rational theory," as an example of pure em-

piricism to be much regretted, not to say reprehended. The following passage, characteristic of an intelligent mind chafing at a paradox, seems so replete with practical suggestion, so applicable to the condition of medicine in all ages, and so especially pertinent to the matter in hand, that I cannot help quoting it :—

“ In respect to the liver diseases of this country, I have no hesitation in saying that mercury is often used in an indiscriminate manner, and with very undefined notions as to a certain specific influence which it is believed to exert over all the morbid conditions of this organ. If the liver is supposed to be in a state of torpor, mercury is given to excite it; and if it is in a state of acute inflammation, mercury is given to moderate the circulation, and reduce its action. Effects the most indefinite, if not contradictory, are also sometimes ascribed to it in regard to its influence on the secretion of bile, and in those affections which are commonly called bilious. Upon the principles of induction with regard to cause and effect, which are recognised in other sciences, it may be doubted whether all these maxims can be right; but I will not take upon me to decide which of them is wrong. I leave the subject, therefore, merely throwing out these doubts, the force of which must be felt by every pathological inquirer, and hazarding the opinion that much of the prevailing doctrine on derangements of the

liver requires to be revised."—*Abercrombie on Diseases of the Stomach*, p. 360.

Another strong argument against the supposition of bile being changed into an aerid or physically irritant fluid is to be found, I think, in a study of the nature of jaundice.

I may as well here observe, and ought perhaps to have premised before, that, by the term "aerid," I only mean such condition of a fluid as would, by simple contact with a mucous membrane, produce, in this, irritation. If bile be absorbed into the substance of an organ, as the brain, it might produce functional disturbance, but that would be no reason for concluding that it had acquired an *acrid* quality. Any extraneous fluid, in its healthiest and most normal character, might have that effect.

But if it can be shown that bile very often, or even very generally, may permeate the substance of important organs, where its presence is altogether exceptional and a morbid indication, without sensibly affecting their natural functions, this would afford a presumption, amounting in my own mind to proof, that, when in simple contact with a mucous surface, and that the mucous surface of the particular organ for whose special use and benefit it is intended, it could not *there* prove aerid or irritant.

I must here so far anticipate what it is my purpose to say of jaundice by stating my opinion that under this common designation have been con-

founded two distinct diseases. Or perhaps it would be more exact to say that jaundice is an indication of two opposite conditions of the system generally, and of the liver in particular; the one in itself a comparatively mild and curable disorder, the other a dangerous and often quickly fatal malady.

The former arises from the passage into the tissues of bile, the product of the hepatic function; and I therefore term it "Icterus cholæus," or bilious jaundice. The other I propose to call "Icterus choloides," or bilioid jaundice, as denoting a similar distribution, *not of bile itself*, but of some or all of its constituents, which the liver, owing to its function being suspended, has failed first to eliminate, and then combine.

The former is evidently an instance of a secretion proper out of place; the latter of a secretion universally *vicarious*. The product of this universal vicarious elimination *in colour* resembles bile: that it should differ, however, in its sensible properties might be expected from its origin; and that it does so is shown in its effects.

In a subsequent chapter I shall have to dwell on this distinction more fully. At present I must content myself, in further illustration, with only referring to the generally recognised fact that under the head of jaundice are included two distinct classes of cases; the one mild, and, *per se*, unattended with danger; the other grave, and, *per se*,

of a very fatal character. The milder type is exceedingly common, the other comparatively rare; but, when present, markedly distinctive. It must, however, be understood that they may exist together, the latter being superadded to the former; in other words, suppression of bile ensuing on simple obstruction.

In the following inference, therefore, I would be understood to refer only to the first, or milder, disease.

In cholic jaundice, then, the bile that has been duly secreted by the liver, instead of passing *more solito* into the duodenum (owing to some one of a variety of obstructive causes that I need not here particularize), is carried by the lymphatics into the blood, and so distributed throughout the system. Under such condition the various tissues may not, by change of colour, indicate an equal infiltration; but we know that all the organs must be subjected to an equal influence by the universal presence of this extraneous element in the general circulation. And yet, considering this, it is wonderful how trifling, often inappreciable, are the results; and even these may generally be assigned to other causes. So much so, that, paradoxical as it may sound, a person may be deeply jaundiced, and yet in perfect health. For, so soon as the cause of the jaundice is removed, and healthy assimilation restored, the disease is at an end, though its product remains,

palpable to sight, but innocuous. In India especially such cases are common ; and I find that they are by no means rare in temperate climates. Dr. Cheyne cites a remarkable instance in which the transudation of bile by the skin was so excessive as abundantly to stain the linen, and even a handkerchief applied to the face ; and yet there neither was, *nor had been*, sensible indisposition.—*Dublin Hospital Reports*, vol. iii. p. 269.

Are such cases compatible with the idea of bile being an acrid fluid ? And, as already urged, if not irritant when thus diffused, is it likely to prove so in its own natural *habitat* ?

One more, and to my mind equally cogent, argument against the probability of bile being ever *secreted* otherwise than a harmless and beneficial product, rests on the circumstance that, whatever may be its character in the gall-bladder—tarry and viscid ; green or olive ; thin and serous ; sparkling with cholesterine ; or otherwise modified by prolonged retention in that sac—its condition in the hepatic ducts, in immediate proximity to its source, is always the same ; namely, that of a yellow soapy fluid, bitter indeed, but at the same time imparting to the taste a bland and soft impression ; in fact, with all the acknowledged sensible properties of healthy bile.

By retention in the gall-bladder, within a certain limit, bile becomes, as already observed, darker and

thicker. The watery particles being diminished, it becomes concentrated; and its sensible properties are at the same time evidently augmented in proportion. So that whereas hepatic bile is essentially bland and unirritating, cystic bile, that has been retained beyond the ordinary healthy interval, acquires an additional stimulating quality; that is, within a certain limit; for, by very long-continued retention within the gall-bladder, even the biliary elements at length undergo absorption, till nothing remains but a thin, serous fluid.

In health, the use of the gall-bladder may be assumed to be that of a reservoir for the accumulation of bile, to be poured out on those occasions when the process of digestion is being actively carried on. Not less interesting, however, appears to me a consideration of its use in disease.

In ordinary constipation (*torpor intestinorum*), for instance, the one thing needful for its removal (as before explained) is a renewal of the suspended or diminished hepatic function. During this interval of acholia (*torpor hepaticus*), the bile in the gall-bladder, by retention, has been undergoing a process of concentration. Simultaneously, however, with a renewal of the liver's action, and to make place for the *new* bile, this concentrated *old* bile flows into the bowels, which are thus stimulated to a peristaltic action beyond the natural action of health, and proportionate to the augmented stimu-

lant quality of this cystic bile. The longer the constipation, the longer the retention within the gall-bladder: the longer the constipation, the stronger the stimulus required, and the stronger thus it will have become.

But it must be borne in mind that elose on this stimulant fluid, and even mingling with it, follows, with gentle and soothing effect, the *new* bile.

Conformably with this view, we observe that the first motions consequent on relief of constipation are uniformly dark, from the presenee of (old) cystic bile; but they are soon, and often with marked suddenness, succeeded by the familiar yellow tinge of the fresh secretion.

In the relief of acute dysentery, too, parallel results ensue. The liver has been similarly, or in a yet higher degree, inert; and the cystic bile is still more concentrated. So soon, therefore, as the liver is unlocked, this cystic bile flows out first—nature's purgative, clearing, as it were, the way—and immediately succeeded by the new bile, bland and unctuous. After the scanty and painful disengagements of bloody mucus, the first sense of relief is simultaneous with that *first* dark, "offensive" motion—which, interpreted aright, means health and safety—often closely followed by others of a lighter and brighter hue.

As in dysentery the scanty, so in cholera the profuse, watery dejections are succeeded—where life

is to be reprieved—by that same dark, offensive-looking, but most welcome harbinger of safety—darker, more offensive, almost tar-like, but indicating nevertheless, a more concentrated state only of cystic bile. And then, if but the vital powers hold out a little longer, our patient is safe.

This, then, I believe to be the chief use of the gall-bladder in disease: to prepare, by retention, a form of bile more concentrated, and thereby more adapted to aid in the correction and relief of existing morbid conditions.

After a purgative dose of calomel, or other cathartic, a feeling of soreness at the rectum is a symptom not uncommon, and it is usual to attribute this to the aerimony of the matter discharged; but it is probably owing as much to a soreness of the part itself, which, after fevers and other inflammatory states, after continued constipation, and even after common catarrh, is subject to an erythematous condition, or sometimes to an herpetic eruption, like that affecting the mouth and genitals, and which of course renders it preternaturally sensitive. In acute dysentery the sensibility of this part is excessive, and yet nothing can be more unirritating than the matters discharged, being merely mucus tinged with blood.

Calomel, with other cholagogues, acts, as shown in a previous chapter, and in the chapter on cholagogues, *electively*, either on the liver or on the bowels. If it

should act on the former, the latter are protected, derivatively, from its irritant effect, and *vice versâ*. It is, however, possible that this protective or derivative effect does not extend to the rectum, or *beyond the limit where assimilation ends*. In such case, the calomel would pass, painless and unirritant, through the whole coil of intestine, till it reached the rectum, or non-assimilative portion. And should that part be already, as suggested, in a morbid condition, we should have no less than *three* assignable causes for pain: the soreness of the rectum, the irritant nature of the remedy, and the increased stimulant property imparted to bile by retention in the gall-bladder.

In an early part of the present chapter (page 56 et seq.) I endeavoured to indicate a general law of the economy applicable to all properly so-called *secretive organs*, by virtue of which, should part or whole of the secretory apparatus become unfitted, through organic change, for the elimination of a beneficial product, the secretive function of that particular portion, part or whole, stops. Conformably with this law, it would seem, firstly, that morbid organic change in the structure of the liver is always, in the first instance, more or less partial or localized; and secondly, that by such change the function of the portion so involved is entirely suspended: not that it secretes vitiated or noxious bile, but that it secretes none at all. The

liver being an agglomeration of *acini*, or lobules, each in itself a complete and independent structure, this consequence is the more intelligible.

On the other hand, it may be urged that, in some morbid conditions of the liver—as in cirrhosis, for example—the whole structure is involved, and that secretion goes on nevertheless. But even admitting this, there is no reason whatever for supposing that bile furnished under such circumstances possesses any injurious quality, though its beneficial property may be, and doubtless is, impaired. A deteriorated secretion may still be better than none. In an advanced stage of the disease, however, a total suspension does appear to take place, no trace of true bile being discoverable in either gall-bladder or ducts. At the same time it is worthy of note, incidentally, that in such cases the evacuations have to the last retained a natural and apparently healthy colour, affording another strong presumption in favour of the production of a faecitious bile from the surface of the intestines themselves.

In structural diseases of the kidney, on the contrary, as has been before observed, the elimination of urine may go on to the very last, liable to an infinity of change in its chemical constitution, to which secretions proper are not subject; but as urine is an *excretion* only, and not destined to any ulterior beneficial use in the economy, any such change is *per se* of no consequence whatever.

But in the history of eirrrosis there are some other features strongly opposed to the supposition of bile ever becoming aerid, or otherwise noxious, the chief of which are the following.

Diarrhœa is not usual in the progress of eirrrosis, which might have been expected had the bile acquired an irritant property: constipation, on the contrary, is an ordinary concomitant symptom. Towards the close, however, of the disease, diarrhœa does set in, often sudden and colliquative. It would seem as though, so long as there was bile furnished by the liver, ever so small in quantity, and ever so reduced in its essential biliary elements, this had still sufficed to keep the bowels, if not efficiently working, at least in a soothed and unirritated condition. But no sooner has the disorganization of the liver reached that point where secretion stops altogether, than irritation is set up in the bowels, increases rapidly, and without further chance of check. Just as in other diseases—diarrhœa, dysentery, and cholera—absence of bile is the proximate and inevitable cause of irritation.

Neither in cirrhosis do we find signs of an irritant agent in the form of ulceration, either in the intestines or in the biliary ducts.

And lastly, it may be worthy of notice that, neither in the liver nor in the hepatic ducts, in this or in any other disease, has bile been found so

changed in character as to warrant a suspicion of its having ceased to be a *beneficial* product, much less that it had acquired any irritant property; for though the biliary elements may vary greatly in their proportion to the aqueous particles, the essential chemical constitution remains the same.

In the gall-bladder bile becomes thicker and more concentrated, doubtless by absorption of the fluid contents; and after greatly prolonged retention in that cyst, as sometimes occurs in permanent closure or obstruction of the cystic or common duct, even the biliary constituents appear to be absorbed, till nothing remains but a pale serous fluid.

With respect to this subject of the unchangeable beneficial quality of bile, I am deterred, by the apprehension of becoming tedious, from entering on a further examination of its nature and sensible properties in other diseases fatal to life, and in which, therefore, we have an opportunity of inspection; but have selected cirrhosis as the most familiar and best understood example of general organic change. I shall therefore only briefly remark, that I have found nothing in the pathology of other and less common diseases of the liver, either within my own experience or as recorded by Portal, Andral, Morgagni, Abercrombie, Budd, and others, to induce a belief that in those diseases hepatic bile ever acquires

any positively noxious quality. With respect to abscess, it has been elsewhere pointed out that the portion of liver not immediately involved continues in the vigorous and healthy enjoyment of its function.

CHAPTER III.

ON CHOLAGOGUES AND THEIR ACTION.

Importance of Liver—Apt to be overlooked—Excess of Bile within the Bowels comparatively harmless—The Secretive Process may, however, injure the Apparatus of Secretion—Injurious results of Over-stimulation—Cirrhosis—Explanation of Nutrition sustained after Function of Liver has expired—Phenomena of Salivation explained—Analogous Eclectic Action of other Medicines—Anomalous Action of Mercurials apparent only, not real—Sedative effect of Calomel in Disease—Liver not very sensible to Pain—Very sensitive to other impressions—Action of a sufficient Cholagogue—Salivation impossible as long as the Liver is acting freely—Necessity of restraining this Action for that Object—Other sources of Restraint or Suspension—A Liver congenitally, or secondarily, inert—Resistance to Constitutional effect of Mercury a sign of a vigorous Liver—Beneficial action of Mercury in Infants—Absurdity of combining Opium except for a specific object.

THE liver is one of the largest viscera. Besides a special circulation in common with other organs, it has an additional supply, comprising all the blood of all the organs concerned in the process of assimilation; its existence in nature is nearly universal, being found in almost the lowest animals: these and other minor anatomical considerations suffice to indicate a very important organ. And this view is amply confirmed by a knowledge of its pathological

history, insomuch that there is probably no disease or affection of any one of the viscera engaged in the great business of nutrition in which it does not act a prominent part. He, therefore, who, in the treatment of this numerous class, overlooks the condition of the liver, or the effect that his remedies may have on its function, neglects to take into account a most essential element of either success or failure.

I have already laboured to show that the notion of bile being an acrid or injurious product is unsupported by any one fact, or any direct observance of its nature; that it is no more subject to such change in its condition than the blood, saliva, milk, semen, or any properly so-called secretion; that it may become thicker, thinner, more or less abounding in its specific constituents, more or less assimilative or nutritious, but, however varying in these non-essential respects, always innocuous.

Taking, then, this view of the innocuous nature of bile as established, it should follow that no excess of it can be prejudicial; and, as respects its effect on the bowels, I believe this conclusion to be just. On the other hand, this rule by no means applies to the presence of its constituents in the blood before secretion, nor perhaps in the tissues after secretion. In its natural place of ultimate deposit, the bowels, bile cannot be otherwise than innocuous *in quality*. If ever supplied in quantity greater than needed for

purposes of assimilation, the excess would probably prove as much a source of irritation as the same quantity of blood, or other bland fluid, and so conduce to its own removal, but no more.

Because a redundancy of bile in the bowels can do little or no harm, it does not, however, follow that the process by which it has been furnished has been equally un hurtful. A gland may be stimulated, and may continue day after day to pour forth an inordinate amount of secretion, that secretion ever, as at first, bland and harmless. But such excessive and continued action must impair the function of that gland; and, if carried beyond a certain point, must induce some form of organic change. And so it is with the liver, which is but an aggregation of simple glands, having their congeners throughout the whole track of enteric assimilation. The natural *stimuli* of air, exercise, food, and healthy blood, suffice to sustain the necessary secretion of bile, without over-tasking the apparatus of supply. Artificial stimulants, whether in the form of condiments or medicine, alcoholic drinks, and the heat of tropical climates, similarly induce a sufficient, or more than sufficient, supply of bile; but always at the expense of the secreting organ, which, by the continuous drain on its powers, becomes daily less competent for its office.

Under such circumstances the individual *acini* may shrink and waste away, which process I take to

be the true history of cirrhosis; their secreting power daily diminishing, and at last ceasing altogether. But in the course of this virtual annihilation, slow perhaps, but irrevocable, the intestines have been accommodating themselves to the gradual privation of their natural measure of hepatic bile, and have been themselves subsidizing a vicarious product, imperfect necessarily, but yet often adequate to sustain life for months, or sometimes apparently for years, *after there is no true liver left.*

Here, in connexion with this gradual impairment and diminution of the liver's function, it seems to me that a very interesting inquiry presents itself, and one which may serve to elucidate one of the most curious and important, and at the same time seemingly obscure and anomalous, pathological problems in the whole history of medicine; namely, that offered by the phenomena of salivation.

And firstly, as to the reason that this result is so uncertain. We know that ten grains of calomel may be given to one person daily for days together without inducing salivation; and that a single such dose, or even a much smaller one, will produce in another the full constitutional effect, or even in the same individual, at some other time, and under different conditions. Not only so, but that twenty grains at once may be given with advantage, or no other obvious effect than ordinary purgation; whereas the same divided into doses of two grains

nightly (uncombined) will in the same individual cause general disturbance, griping, and possibly eventual salivation. These apparent inconsistencies I hope to reconcile.

I would first refer to a well-known property common to many medicines; namely, a tendency to act on particular organs, though taken all indifferently into the stomach—diuretics on the kidneys, ergot on the uterus, digitalis on the heart, strychnine on the nervous system. Blisters again, though applied outwardly to the skin, will sometimes act specially on the genito-urinary organs. But, besides this, it is worthy of note, that many, perhaps all remedies, if prevented, by the absence of certain conditions, from acting on one particular organ, will transfer their action to another. So certain drugs will act on the skin, as diaphoretics, if the patient be kept warm; but, if this condition be withheld, on the kidneys, as diuretics. Others act as emetics in large doses; in small ones as diaphoretics or expectorants; in intermediate doses again, frequently repeated, as purgatives; tartar emetic, for example. Turpentine in a full dose is a purgative only; in small ones a diuretic. And other examples will occur to the reader. In this general character, then, that the same medicine will sometimes affect one organ, sometimes another, it is evident that there is nothing peculiar to mercurials.

But in the case of calomel there does seem a

special and peculiar anomaly, consisting in the well-known fact, that in small doses it proves irritant, and in large less so, or not at all. This result, however, I believe to be apparent only, not real; and that virtually its action is perfectly analogous to that of the examples just cited, and in accordance with the general law thence deduced.

The fact I take to be simply this: that calomel in small doses acts on the bowels; in large doses on the liver. And this elective property furnishes us with a key to all the results, hitherto seemingly paradoxical, of this important remedy.

Accordingly, if to a healthy adult one or two grains of calomel (alone) be administered, the probable result will be irritation of the intestinal canal, with some pyrexia; nausea, with griping pains; and one or more unsatisfactory evacuations, passed with slight tenesmus. If to the same individual, under like conditions, ten grains or a scruple be given, the result, on the other hand, will be little or no constitutional disturbance, much less pain and griping—often none until the bowels are about to be relieved, and then the stools will be free, copious, bilious, and without pain.

Analogous, though of course modified, results will ensue if the same respective doses be used in constipation, diarrhœa, dysentery, cholera, and other allied disorders; with, however, this additional and noteworthy feature in disease, that the action of

the large dose will be accompanied by a direct sedative effect on the nervous system, often immediate, and signalled by a subsidence of previous pain.

In health, when the small dose is taken, the bowels are irritated, become painful, and, as a measure of relief, pour out mucus, mingled probably with a little *enteric* bile. Thus the calomel is peristaltically shifted from one part of the intestine to another, causing renewed irritation at each step of its course, until finally discharged. Thus, too, is occasioned a partial and ineffectual diarrhœa, rendering perhaps necessary a subsequent dose of oil or some other purgative.

When, however, the larger dose is taken, the liver is the organ *electively* and exclusively acted on (in keeping with a law very common in therapeutics, as already shown). Hence the absence of pain. For, even when inflamed, the liver—that is, its substance—does not evince much sensibility of this kind. Moreover, it possesses within itself, when its function is still vigorous and unimpaired, the means of immediate relief, in the outpouring of its secretion. There is no more familiar occurrence than this relief of nervous irritation by secretion: toothache, varieties of neuralgia, and other forms of local pain, are thus often assuaged, the means of relief being recognisable by swelling of the part. In *this* respect, and in some others very remarkable, to be

noticed hereafter, the healthy liver is unsurpassed in sensibility by any organ of the body. Stimulated, then, by the presence of a *sufficient* cholagogue in the alimentary canal, it begins to secrete. Simultaneously, to make way for the new bile, the contents, more or less accumulated, of the gall-bladder—yellow, or green, or olive, or seemingly black, according to the duration of their retention—begin to flow. The mingled streams pass through the common duct over the surface of the intestines, which themselves, if previously congested (through repletion and hyper-cholerization of the portal system), are now simultaneously relieved by the subsidence of those causes, furnish forth their own secretions, and institute an equable, beneficial, and comparatively painless peristaltic action. Thus ensue stools varying from yellow to black, or successively exhibiting, in inverse order, the shades of colour above just enumerated.

So long as the liver is thus capable of responding to the demands on its powers; so long as its sensibility and functional capability remain unimpaired; so long as it is thus competent to relieve itself and the system of a specific poison; we may administer calomel with impunity or benefit. No salivation or other constitutional injury can possibly ensue, so long as our great safety-valve is efficient and working. But this condition is liable to be deranged by a variety and complication of causes,

the chief of which I propose to show, and the recognition and avoidance of which will tax to the utmost the skill and acumen of the physician.

If a dose of calomel, too small to act as a cholagogue, be given, it will (under this inexplicable, perhaps, but no less undoubted law of election) act on other organs, none of which may possess equal powers with the liver of resistance and relief. In some idiosyncrasies a single such dose will affect the salivary glands, and produce the well-known associated symptoms of mercurial poisoning; and in most habits a repetition of small doses will do so. In the meanwhile, the more freely the liver is acting, the longer the effect will be delayed. If we desire to expedite it, therefore, we must restrain the liver—put a log upon its action.

One mode of doing this is by combining, with our small doses of calomel, opium. The liver is very susceptible of the action of this drug; though habit, as in the case of opium-eaters, will of course blunt this sensibility. But in an ordinary stomach a very small dose suffices to place a temporary check on the secretion of bile. By thus then restraining the (otherwise *derivative*) susceptibility of the liver, we shut off our cardinal valve of safety, and the poison that might otherwise have expended some of its force on this elastic organ is driven—by the usual law of elective action—to exert all its virulence on the stomach, and, through it, on the

system generally. In the empirical knowledge of this fact our art has hitherto possessed a valuable resource; with the attainment of a more rational knowledge, we may avail ourselves of it with greater confidence and safety.

Similarly, if whilst mercurials are being taken, the action of the liver be restrained or abruptly arrested, by cold, astringent medicines, errors in diet, depressing passions, sudden and violent emotion, or by any other conceivable cause, salivation may be precipitated.

Or, again, the liver may be constitutionally sluggish and inefficient, as in serofulous subjects; or its natural sensibility may have been impaired through excess of stimulation, whether by means of diet, abuse of purgatives (especially of the cholagogue class), tropical heat, sedentary habits, or by a complication of such causes; by disease, chiefly of the structurally degenerative kind—for in certain inflammatory conditions, as will be seen, its sensibility to mercury is actually increased; by natural decay; and by all sources of general debility. Under any of these conditions the door of safety has been reduced, and the alternative of a counter-elective action rendered the more probable, together with the usual irritative and constitutional results.

It is interesting, moreover, to note that in this various effect, on individuals, of mercury we possess a sure and valuable index of the state of the con-

stitution, as respects the vigour of the liver in particular; and, as dependent on that great organ, of the digestive function generally. In tropical climates especially, susceptibility of the beneficial operation of mercurials may be always regarded as indicative of a sound constitution, and of digestive organs that still retain their pristine tone and freshness. On the other hand, old residents, long subjected to the combined effects of heat, sloth, and erapulence, require larger doses to insure an *elective* action on the liver.

In further illustration of this principle, it will be found that there is no subject on whom an adequate dose, say two grains, of calomel acts with surer beneficial result than on a young and constitutionally healthy infant. This accords with experience in all climates. In the various alimentary irregularities of young children, a comparatively large dose acts, as Dr. Davis, in his "Diseases of Women and Children," has observed, "like a charm."

Calomel should never be prescribed with opium, excepting with a distinct view to restrain its action on the liver, and so determine it to some other organ or organs. I regard this as a golden rule. But I need scarcely say that no such precept is recognised in practice, or is to be met with in our text-books. On the contrary, whenever a full dose of calomel is determined on, it is the fashion to conjoin a little opium, not with a definite idea of *modi-*

*fy*ing the result, for that would have some show of reason, but with a vague and timid notion of *limiting* its effects, lest its action should prove too powerful. As if one should administer a eup of hot water with one hand, and a modicum of cold with the other. If twenty grains of calomel thus require a grain of opium to control them, does it never occur to the prescriber that his object might be as well attained by reducing the dose to ten?

From all my experience of the use of these most important remedies, I cannot conceive any circumstances of disease in which it can be necessary or advisable to combine opium with calomel, except when we desire a constitutional or alterative effect. Nor can I recall a single satisfactory example of salivation in the absence of one at least of *two* conditions—the *first*, a conjunction with opium; the *other*, a liver congenitally or secondarily inert.

In cases of death from hepatic abscess, it is interesting to observe how unequivocally healthy often appears that portion of the structure not immediately involved. It would even seem as if it had been endowed with an unusual degree of efficiency, to compensate for the destruction of the remainder. But what is still more interesting to note is that the sensibility of this healthy portion also appears to be intensified in a remarkable manner. Annesley long ago was struck with the difficulty, almost amounting to impossibility, of producing salivation in such

eases, insomuch that he regarded this resistance as strongly indicative of the existence of hepatic abscess; and he imputed it not to increased, but to diminished, sensibility of the liver. But I trust that enough has been said to show that such a pathology is diametrically opposed to truth, and that it is when mercury acts on the system through the intestines, not when it acts on the liver, that its constitutional effects are displayed.

I will offer only one more suggestion in reference to this subject. The liver not being, in its nature, an absorbing or assimilative organ, and the digestive canal being so, essentially and pre-eminently, that mercury, or any other substance, should find its way into the system through the medium of the former, rather than of the latter, is, on this ground alone, so unlikely, so difficult to conceive, and, withal, an hypothesis so unnecessary for any purpose of theory or explanation, that it seems not a little strange that it should ever have been seriously entertained.

As long as any portion of healthy, vigorous liver remains, retaining its native sensibility to impressions (or even, as is often the case, in an exalted degree), so long will this portion suffice, by an increased action and outpouring of bile, to resist the action of mercury. But, as repeatedly stated, when the sensibility of the liver generally is impaired, or when no part of it remains (as it *does* in abscess)

endowed with a preternatural sensibility, then mercury, instead of acting on an organ no longer susceptible of its action, acts, counter-electively, through the medium of the digestive canal, on the system generally, and on the glandular system in particular, being at the same time taken up and circulated throughout the tissues.

Sir James Annesley describes some very interesting experiments that he made on the action of calomel on dogs, in which he was surprised to find that the larger the dose administered, the fainter were the traces after death, in the intestinal canal, of inflammation or irritation. This was by him regarded as a fact not admitting of any satisfactory explanation; but to my mind these experiments convey an interesting and forcible illustration of the principle we have been considering; namely, that the larger the dose of calomel, the less will be its irritant effect on the bowels, and the greater its counter-elective action, stimulant merely or highly irritant, on the liver—so long as all or any portion of the hepatic *acini* remain healthy, and retain their native susceptibility. An excessive dose of calomel may indeed damage such a liver, but not, excepting mediately, the bowels.

CHAPTER IV.

JAUNDICE.

Icterus Cholicus, and Icterus Choloides—Their Pathology, and their Import in Disease.

I. FROM what has been said in the preceding chapters, it will be understood that I suppose the digestive canal, in its assimilative portion, to be the depository of two kinds of bile ; the one true, or “ hepatic ” (cystic)—the other factitious, or “ enteric ; ” the former bland, unctuous, soothing, or gently stimulating, except after long retention in the gall-bladder, when, by a beneficent provision of Nature, its stimulating property is enhanced ; the latter possessing many of the sensible properties, and, doubtless, salutary uses of the former, but still a substitute only, more or less inefficient, and, sometimes at least, as in certain forms of bilious diarrhœa, possessing a positively acrid and irritant property.

It has also been pointed out that, in some forms of diarrhœa, and in so-called “ chronic ” dysentery, the motions may be conspicuously bilious, and yet at the same time the liver totally inert, requiring,

as the only means of cure, a renewal of that interrupted function. Instances have also been cited in which, from structural degeneration, or other physical causes, the passage of bile into the duodenum had been rendered, for many months before death, impossible, and yet in which, during all that period, the evacuations had presented an appearance more or less yellow or greenish. These and other familiar facts, both in the pathology of disease and in the effects of our remedies, have been adduced, all concurring to support the supposition of an enteric secretion of bile as a common and ordinary occurrence, and indeed not admitting of explanation, excepting on the strength of such a supposition.

The various glands with which the assimilative portion of the digestive tube is studded have also been assumed as probably subsidiary to the liver; perhaps themselves, some or all, rudimentary hepatic *acini*, only having their nature and functions modified by a different position, and by their different relation to that great source of bile, the portal system.

But though, from their structure and position, the intestinal glands may be supposed to be the only organs endowed with this subsidiary, or vicarious, faculty of *secreting* a pseudo-bile, we know that there is scarcely any tissue of the body in which, under exceptional circumstances, bile may not be found. Still it cannot be reasonably imagined that these various tissues have special secre-

tory powers in common with, or at all resembling, those possessed by organs that come under the common definition of "ducts invested by capillaries." It is casier to imagine, as indeed is very generally supposed, that hepatic bile—that is, bile that has been duly secreted by the liver—failing to find its natural passage into the duodenum, has been conveyed into the general circulation, and thereby distributed throughout the tissues, in which it is deposited, probably, by some process of simple exosmosis. And it is particularly worthy of note that this deposition of "true," or hepatic, bile does not appear, *per se*, to be attended with any injurious or irritant effect whatever; true bile being essentially a nutritive fluid, bland and unirritating, as are all properly so termed secretions, being products of glands, and "destined to beneficial use in the economy."

But, just as we find that the factitious bile supplied by the intestines possesses, sometimes if not always, an irritant property, so we should expect to find that any other form of spurious or factitious bile, in any other part or tissue of the body, should similarly possess irritant or otherwise noxious properties. Moreover, if a false bile should have this irritant effect in the natural home and *habitat* of bile, much more might it be expected to display such an effect when deposited in tissues, in which the occurrence of bile at all is to be

regarded as an entirely exceptional and morbid phenomenon.

And both the above expectations are borne out by facts. For there is another class of cases of jaundice, quite distinct from those already alluded to, in which a "bilious" tinge may pervade the body, and in which, nevertheless, we are well assured that the symptoms are not due to hepatic, or cystic, bile at all. Such are those cases, common enough, in which the function of the liver has been gradually annihilated by organic disease; or those others, equally familiar, in which that function has been either abruptly and suddenly arrested, or has more gradually ceased. Under all these circumstances alike, it is evident that the constituents of bile, remote or proximate, not having been separated by the liver, must accumulate in the blood, inducing a condition of "hyper-cholcrization." These biliary constituents may subsequently become outwardly manifest by their deposition in the tissues in the form of jaundice; or they may remain in the blood, unseparated, and, by their presence therein, cause various forms of irritation, as witnessed in diarrhœa, dysentery, and cholera. Or, again, it is possible (and it seems not unworthy of note) that these may sometimes be deposited in the tissues, without being evident to sight; for, though bile itself is yellow, its uncombined (or otherwise combined) elements may be of a totally different hue, or even colourless.

Thus might we have all the conditions of jaundice (using the term for want of a better) without the colour. May not such a state, in a greater or less degree, obtain in diarrhœa, dysentery, and cholera?

One thing appears pretty certain: that, whether these constituents of bile, *that have not undergone "hepatization,"* remain in the blood, or are deposited therefrom in the tissues, they are inevitably and eminently injurious. As also explained in a preceding chapter, besides inducing other well-known phases of cholæmic poisoning, their presence in the blood constitutes an important part of the pathology of diarrhœa and dysentery; and (when conjoined with anuria, or suppression of urine) of cholera. Hence, too, in their elimination in these various diseases, consists an important part of the curative process of Nature.

Thus there appears to arise a natural distinction of all forms of jaundice into two great classes; one, in which the bile has been duly secreted by the liver, and subsequently distributed throughout the body, instead of being poured into the duodenum; another, in which the biliary constituents have not been thus combined by the liver, and consequently pass on into the general circulation in some abnormal and highly morbid condition. This latter is a state of "acholia;" and acholia produces "cholæmic" poisoning, just as anuria produces uræmic poisoning.

The former I would designate "*Icterus cholicus*;"

cholic, or bilic, or bilious jaundice; and the latter, "Icterus choloides," choloid or biloid jaundice.

I need scarcely observe that some such distinction has already been in a measure recognised, having been first pointed out, I believe, by Dr. Alison; and what I here term choloid jaundice has not inaptly been designated "pseudo-jaundice." That, however, a state of intense acholia may exist; that the blood may be teeming with the uncombined elements or principles of bile, without their *visible* deposition in the tissues, or other such conspicuous manifestation of their presence; and that this is the case in certain diseases, and eminently so in dysentery and cholera,—is a pathological fact which, however intelligible, has not, as far as I know, been recognised in its full relation to disease, nor as its practical importance deserves.

My main purpose, therefore, in the introduction of the present chapter, is not to give a complete account of the pathology and treatment of jaundice in the manifold forms in which that protean disease—or rather, phenomenon of disease—presents itself (for to do so would require a separate treatise, equal, perhaps, in extent to the present volume), but to introduce the subject incidentally as it were, in connection with, and so far as it forms a part of the etiology of a set of diseases that are naturally associated, and which (from the circumstance of a state of acholia, partial or complete, being common to them all) may be termed "acholic" diseases.

The more important and familiar of these form the chief subject of the present work, being cholera, dysentery, and diarrhœa. I shall limit myself, therefore, to such additional suggestions only as appear to me further calculated to elucidate the pathology of acholia, and more particularly as that condition presents itself in choloid jaundice.

II. All the organs of the body have a marvellous faculty of resistance and repair, if only time enough be given them. The practised opium-eater will consume with impunity as much of the drug as would kill half a dozen strong men, his brain having by degrees accommodated itself to the poisonous effect. A very small quantity of blood lost quickly and suddenly will induce syncope, and even death itself, whilst double or treble the amount may trickle away without any such consequence. In structural diseases of the liver, whercin the hepatic function gradually and almost imperceptibly deteriorates, till at length it is extinguished altogether, existence has been prolonged, and pseudo-bilious motions have been passed for weeks, or even months, subsequent to that extinction—a fact revealed after death by the condition of the parts: the liver scirrhus, tuberculated, eirrhose, fatty, indurated, pale, atrophied; the gall-bladder and ducts wasted and empty, or containing only a thin, serous fluid.

But it is far otherwise when the function of the liver is *suddenly* arrested. There is no time for the system to accommodate itself to the change—no time for the establishment of a vicarious secretion from the intestines—no time for the brain and ganglionic system to become habituated, as it were to a new and powerfully poisonous impression.

Thus some of the worst and most rapidly fatal cases of jaundice are those that ensue on paroxysms of mental emotion. In these coma quickly supervenes, sometimes furious delirium.

Dr. Budd, in his work on the Liver, p. 183, gives a case in which, from the state of the liver and ducts, as seen on dissection, it was evident that there had been no secretion of bile for a considerable period before death, and in which, nevertheless, there had been no head symptoms, and no disorder of intellect. I have seen cases presenting all the leading features of that cited by Dr. Budd, and many others will be found in books. But, so far from viewing them as furnishing evidence of the innocuous nature of biliary principles in respect of the brain, I have rather regarded such cases as affording strong examples of this remarkable power of organs, and especially of the brain, to accommodate themselves to the most powerful influences, if these be but slight in their origin, and by imperceptible degrees intensified. For the degenerated state of the hepatic tissue, though proving the total extinction of

secretion, testified no less to the fact that that process had been slow and gradual. Just as in the case of the opium-eater, the brain had, by insensible degrees, become habituated and insensible to the impression of a powerful poison.

Every one who has seen cases of cirrhosis has remarked the comparative infrequency in that disease of visible jaundice. The stools too are not, as far as my experience goes, so often pale or clay-coloured as they are dark, dull, and dingy ; of a dull yellow occasionally, but never with the lively hue of health. And yet, from the pathology of the disease—the liver undergoing a continual process of degeneration, and the secretion of bile diminishing *pari passu*, and, within perhaps some weeks of death, ceasing altogether—such absence of jaundice is scarcely intelligible, otherwise than on the ground that the intestines have had time to accommodate themselves to the growing deficit of true bile, and, at the same time, to furnish a factitious substitute.

III. The pathology of serpent-bites also, a subject that has always been involved in much obscurity, may, perhaps, receive some elucidation by their being regarded as analogous to the above—as, in fact, instances of cholæmic poisoning. Jaundice is a common feature in these cases, indicating the sudden interruption of the flow of bile in its natural course, and its re-absorption into the circulation.

But, besides this, there is strong evidence to show the joint presence of the choloid species of jaundice; that is, of a state of acholia, or total *suppression*. This view is supported not only by the intensity of the shock sustained, and by its fatal tendency, but also by the circumstance that, when these wounds prove fatal, the gall-bladder is invariably found distended with thick, dark bile—an unfailing sign that the liver's action has been for a while suspended. For it should be borne in mind that cholic and choloid jaundice are often thus co-existent, *retention* being immediately succeeded by *suppression*; but the former is comparatively a trifling ailment, and becomes altogether insignificant and subordinate in presence of the graver derangement.

Jaundice, however, in its ordinary sense, is not externally manifest in all cases of even fatal snake-bite. But, though *deposition* be a positive sign of the presence of biliary elements in the blood, its absence does not furnish even negative evidence to the contrary. For, as before observed, the uncombined principles of bile have none of the characteristic colour of that secretion; and the blood may therefore be teeming with these, without any such evidence to be found in either the skin or conjunctiva. And, in fact, as in cholera and in acute dysentery, so also in serpent-bites, and perhaps also in those instances cited of jaundice consequent on mental emotion, such cases constitute the worst

forms of eholæmie poisoning. For, as just said, discoloration is merely a sign of a comparatively unimportant affection; and, until after death, I do not know that we possess any positive indication of its more formidable eongener, except such as is derivable from the extreme urgeney of the objective symptoms, as còma, delirium, and convulsions, results of cerebral irritation, on the one hand, or in the form of intestinal irritation (as in dysentery and eholera) on the other.

IV. I have had occasion to notice, in the majority of cases of jaundice of either variety, that recovery has been signalled by one of the same dark, tar-like evacuations that I have elsewhere described as critical, both in dysentery and eholera. And I am disposed to conclude that some such indication forms an essential prelude to recovery in every case of jaundice, though, in the absence of attentive observation, it may be overlooked. Or perhaps, in the lighter forms of cholic jaundice, where hepatic or cystic bile has been re-absorbed, without complete or very temporary *suppression* only, the retention of bile within the gall-bladder would not be long enough to impart to it the characteristic green, or olive, or almost black hue. At the same time it is right to state that I have also known these tar-like evacuations appear shortly before death. But, under such cir-

cumstanees, I have always been of opinion that they no less indicated a sanative effort on the part of the system, which would have proved successful if the powers of life had only lasted longer. In the same way, though a favourable sign in cholera, death is often immediately preceded by one or more of these dark motions, and I have recognised the same appearance *within* the bowels after death. But if neither in the motions during life, nor within the intestines after death, the dark, concentrated secretion is certain to be found in the cyst itself—I believe in every fatal case of cholera.

CHAPTER V.

ACUTE DYSENTERY.

Its Pathology, Symptoms, and Treatment.

ACUTE dysentery is essentially a disease of constipation.

With this axiom, which at first sight may appear paradoxical, I have headed this chapter, not only because I am convinced of its correctness, but because I believe it to express a truth that should never be lost sight of in the treatment of this common and dangerous disease, "the scourge of fleets and armies." To its illustration I shall proceed forthwith.

In that form, common in all tropical climates, and which may be regarded as typical, the following are the more essential features. The first warning of a coming attack is in irregular action of the bowels: either they are lax or confined, with pains in the abdomen generally. These pains are neither constant nor fixed, nor are they augmented by moderate pressure, but are rather spasmodic twinges, not very severe, intermittent, and recurring at pretty regular intervals, sometimes of a few minutes.

There is, however, even in this premonitory stage, some tenderness often discoverable, by pressure with the points of the fingers, in the iliac regions, but especially on the left side. There is generally no pain in the region of the liver, either in this premonitory or in the first and acute stage. At the same time, the patient is moody, ill at ease, disinclined for exertion, and without relish for food; often there is a distaste for those things in particular, as beer or tea, that have constituted in health the favourite articles of diet. But this fastidiousness is perhaps incidental to all forms of dyspepsia.

By restriction in diet; by recourse to a purgative, emetic, or, in fact, any other medicine calculated to arouse the dormant action of the liver; by timely removal from malarious influence; often by warmth alone, especially about the epigastrie and hypochondriac regions; sometimes spontaneously; these premonitory symptoms will subside.

It may here be worthy of note that a person in this condition is not only predisposed to dysentery, but also appears more liable, comparatively, to be the victim of cholera, should that epidemic be prevailing at the time. Or, it would probably be more correct to say that, whether the system be engaged in a contention with the exciting cause of cholera, or with dysenteric influences, the same train of irritative phenomena are awakened—the contest is

waged by the same means. Recurrence to this subject will be found in the chapter on cholera.

The next and active stage is marked by an aggravation, often sudden, of all the previous symptoms, with the supervention of others characteristic of the disease. The skin becomes warm, and fitfully moist; the pulse is wiry and accelerated; the mouth is dry, and the eye glistens. There is increasing anxiety, with complete anorexia; the secretions generally, including the urine, are diminished; and occasionally there is dysuria, but never complete *anuria*, as in cholera. But the condition of the bowels constitutes by far the most prominent symptom, engaging all the thoughts and attention of the patient. The occasional twinges are now replaced by tormina, culminating in intensity on every call to stool. These calls are so incessant, that the patient sometimes passes half his time on the night-chair, in futile endeavours to free the highly inflamed and sensitive rectum of a little mucus, tinged with blood, which in itself is perfectly bland and unirritating; and yet the passage of this has been described by a sufferer "as the passing of molten lead."

At this time the function of every organ immediately concerned in the business of assimilation appears to be arrested. There is, as regards them, if I may say so, a universal "*constipation*." Could we examine the interior of the intestinal tube, we

should probably view a comparatively dry and glistening surface, with perhaps here and there a few scybala, the remains of a former deposit. This surface would also be preternaturally vascular and congested throughout, with the signs of ineipient inflammation more and more manifest as we descend, till they became intensified in the rectum. The various glands too—some at least among them rudimentary livers—would be seen similarly turgid and congested, ready, presumably, to take on a new, morbid, and vicarious action. With this, imagine a liver dry, hyperæmic, and inert, with a gall-bladder of bile already, by retention, olive or dark green, and between this and the irritated intestines a portal circulation preternaturally distended, and teeming with the accumulated elements of bile; and we shall have formed a partial indeed, but probably not untruthful, conception of the pathological conditions with which we have to deal.

But the inflammation of the bowels may, if the primary source of irritation be not removed, go on to suppuration and ulceration; the discharges, at first mucous, becoming purulent, with more or less admixture of blood. During this change, however, the tormina and tenesmus have become less severe; and a little thin, feculent matter begins to appear in the stools. Gradually the quantity of this feculent matter increases, and it assumes various shades, from a dull brown to a dingy yellow: sometimes the stools are clay-coloured; but rarely, I believe,

except when, from ignorance of the pathology of the disease, opium or direct astringents have been employed. As the dejections thus become more feculent and *pseudo-bilious*, the patient experiences considerable relief from his more urgent and painful symptoms; the appetite returns, and, though capricious, may be for a few days even excessive; whilst hopes of speedy recovery are indulged—hopes too often destined to disappointment, for the *acute* has now subsided into the *chronic* stage, and chronic dysentery is by far the more dangerous disease, whilst its treatment is infinitely more difficult and uncertain. The tongue, which in the acute stage may have been clean, has now become of a pale brown; the complexion assumes a pallid, sallow hue; the cornea are preternaturally white; the appetite gradually gets worse, and at last fails altogether; the strength decays; and the mind is gloomy, irritable, and capricious. In this condition, with considerable fluctuations, and a succession of disappointments, the patient may remain for weeks, or even months. Its fuller consideration must be reserved for a succeeding chapter; but there is, in truth, no line of demarcation between *acute* and *chronic* dysentery, and it is therefore to be hoped that this account of the passage of the one into the other will not be considered here out of place.

It is very common, when the preliminary stools of mingled blood and mucus are succeeded by the secondary diarrhœa, for scybala, sometimes in large

size and quantity, to pass together with the fluid dejections. These, however, have no direct pathological connexion with this stage of the disease, but were formed and accumulated during some former interval of partial constipation, short, perhaps, and unnoticed by the patient, and that may have immediately preceded the first evident or pathognomic symptoms.

There is one form in which dysentery occasionally presents itself, in which there is diarrhœa *apparently* from the commencement, and from my own experience I should say more dangerous and fatal than the ordinary and typical kind I have described. Its malarious origin, too, is more strikingly evidenced, being generally met with in low and marshy districts, where intermittent is endemic. A similar type sometimes displays itself, with uncommon virulence, in bodies of troops, predisposed to disease by overwork, privation, and unwholesome diet, and also after confinement on shipboard. Under such circumstances discharges of blood with the stools often form a striking feature, indicating a prevalence generally in the force of a scorbutic taint.

In this form of the disease, however, the difference is apparent only. Every kind of dysentery, it must be remembered, and even ordinary and prolonged constipation, is succeeded, sooner or later, if the patient survive, by diarrhœa. In this severer form of dysentery, the reaction on the part of the

bowels, and the consequent diarrhœa, supervene so rapidly, that the preliminary stage is overlooked. Probably the suspension of the hepatic function has been both more sudden and more complete; and the hyper-cholericized blood of the portal system, therefore, also more powerfully and *suddenly* irritant to the bowels. For, as already observed, organs have a wonderful power of self-adaptation to sources of irritation, when these are slowly applied. The analogy is well borne out by a reference to the pathology of cholera, in which disease the suspension of the biliary function is not only still more sudden and complete, but the effect is enhanced by simultaneous suppression of the renal function; accordingly, the reaction on the part of the bowels is sudden and violent, and the flux proportionately profuse.

In concluding this sketch of the symptoms of acute dysentery, it may be as well to guard against the possibility of misconception, by pointing out that two distinct periods of "*constipation*" have been referred to; one during the incubative or premonitory period, the other marking the actual invasion of the disease. The first is a simple state of costiveness, often overlooked; the second is a similar and aggravated "*costiveness*," with pain, fever, tenesmus, and scanty disengagements of mere mucus, blood, or pus. The first is commonly succeeded by an interval of diarrhœa, or irregular

action of the bowels, indicating reaction, and an endeavour, often successful, to avert disease. This reaction does not, however, always occur, or is not apparent; in such case the first condition of constiveness is at once succeeded by the actual disease. The apparent discrepancy in the fact of the characteristic symptoms being preceded sometimes by a constive condition, sometimes by diarrhœa, is thus explained.

At this stage of illustration it is scarcely necessary to suggest that the term "constipation," as applied to the acute stage of dysentery, is to be taken in a pathological sense only; but this pathological condition is so important, and so necessary to be impressed on the mind of every one who may have to treat the disease, that I hope to be pardoned for dwelling on it so much. In theory it may be even intelligible enough; but when one patient is passing frequent stools of blood-stained mucus, another quantities of blood itself, and a third, perhaps, stools of abundant purulent matter, it may be difficult, especially for the young practitioner, to realize the fact that in all these the pathology is essentially the same, and therefore that the principle of treatment must also be the same.

During the premonitory period, the disease, by judicious treatment, may be certainly arrested; but the medical attendant has not always the opportunity. At this stage the conditions are, on the

one hand, an inert liver (acholia) ; and on the other hand, and as a consequence, bowels either in a state of dry congestion, or irritated into a fitful, irregular, and possibly, in part, vicarious action : between them a congested and hyper-cholerized portal circulation. Under these incipient circumstances the native sensibility of the liver is still unimpaired, and it readily responds to an adequate stimulus. So that whether we prescribe calomel, or oil, or senna, the chief and immediate effect is cholagogue. The liver is unlocked ; the portal vessels relieved, not only as respects their congestive or hyperæmie state, but of their biliary elements as well ; and the bowels, no longer in contact with an abnormal and irritative circulation, subside into the calm of health ; at the same time the new bile flows soothingly over their surface ; and thus harmony is restored.

It is, however, in the next or more acute stage, that recourse is more commonly had to professional aid. The condition of acholia, or *torpor hepaticus*, is now more complete ; the hyper-cholerization of the portal vessels increased ; and—from a combination of these two causes, absence of secreted bile and irritative blood—the inflammation of the bowels intensified. The previous *torpor intestinorum*, or the fitful diarrhœa, is now replaced by a highly vascular, comparatively dry, and acutely sensitive state of the whole alimentary canal, but especially towards its lower part, whence exudes a scanty

mucus tinged with blood. The ordinary constitutional symptoms of irritation are also present, and the secretions generally are diminished, including the urine; this, however, is never, as in cholera, wholly suppressed.

As in the premonitory, so in this confirmed but yet early stage of dysentery, there is but one essential therapeutic indication. *Unlock the liver.* Read what Dr. Johnson, in his classical work on Tropical Diseases, records of the disease in his own person; how after ten grains of calomel, once repeated, he passed from a condition of intolerable suffering into one of absolute ease and comparative bliss. The same chain of results will follow, already detailed in the treatment of the premonitory stage, and which, therefore, need not be here repeated.

Unlock the liver: whether by calomel, castor-oil, tartar emetic, ipecacuanha, or other last new remedy in fashion—emetic, purgative, deobstruent, but cholagogue withal—the one indication must be carried out. In collateral respects these various remedies may differ *toto cælo*, but in this one of renewing the biliary function they must concur, or be worse than useless. I believe there is nothing more likely to lead to the discovery, in medicine, of important and practical truths, than a comparison of different and seemingly opposite paths, thus leading to one common goal.

In this stage of dysentery, and in most cases, I regard calomel as the surest and speediest remedy.

As, however, stated in a previous chapter (*vide* Chapter III.), the contra-indication to its use is a liver congenitally sluggish—as in strumous habits—or that has become so from the effect of disease, or through continued over-stimulation, whether by medicine, alcohol, tropical heat, or other excess. For should any of these influences have been at work, the liver will not possess, or will have lost, that sensibility characteristic of the fresh and vigorous organ. It will not respond to our cholagogue intent; the calomel will not act, as we desire, *electively* on the liver; and if not (as explained in the chapter on cholagogues) it must act, *per contrà*, electively on the bowels; not only adding to their present local irritation, but with the risk as well of producing that specific constitutional irritation which, under present circumstances especially, is so much to be dreaded. Such instances constitute the exceptions in which calomel “has not been found to answer,” and that have thrown unmerited discredit on a potent, but, in discriminating hands, no less useful remedy. To which must be added those still more numerous cases in which the therapeutic effect has been defeated, diverted from the liver to the bowels, by the unwise addition of opium.

Moreover, by thus stimulating the already inflamed bowels, we incite them to redoubled efforts to free themselves of an irritant poison. They commence to secrete abundantly; the enteric

glands, too—those “rudimentary lobules of the liver”—prepare to take on a new action, secreting, not *hepatic* bile, but an imperfect and vicarious substitute, perhaps itself irritant, as true hepatic bile never is. Thus we shall have hurried on the disease, and precipitated the dawn of that next, and yet more inveterate stage, known as “chronic” dysentery, in which the scanty mucous dejections are replaced by pseudo-bilious stools, with an accompaniment of bloody mucus or purulent matter.

This, then, the presence of an insensitive liver, is the true cause that calomel sometimes fails to cure. But such cases are rare. For, in the absence always of an advanced condition of cirrhosis, or of other structural change, it is seldom that it is so congenitally deficient, or has become so artificially impaired, but that it will respond to a sufficient dose of our most powerful cholagogue. Ten grains of calomel, repeated at intervals of six hours, will, I believe, after the second, or at the most after the third dose, act *electively* on ninety-nine livers out of a hundred; and it will be for the careful and sagacious physician to recognise this exception.

Should he, in obedience to fashion, or from a vague notion of lessening irritation, think fit to combine half a grain or more of opium, it is very likely, with so many chances in his favour of having a good liver to deal with, that the calomel will still act on it *electively* in spite of the opium. But,

should his make-weight prove a little too strong, and just suffice to render the liver insensible to the beneficial action of its fellow, that versatile drug will assuredly act on the bowels instead. Then will ensue increased irritation, preeipitation of the disease in its course, and, possibly, salivation.

On one occasion I prescribed for a powerful convict, who was passing nothing but slime and blood, a scruple of calomel. The following morning I was surprised to find that he was worse, and still passing stools of the same character—nothing but blood and slime ! Attributing to personal idiosyncrasy this unusual absence of a purgative effect, I resorted to a treatment by tartar emetic alone, and in a few days the man was doing well. It was subsequently elicited, by the dresser's own admission, that, thinking to improve on my prescription, he had added to the calomel two grains of opium.

As already said, two or three doses, sometimes one, will ordinarily suffice to open the liver. The result, especially as respects the sensations of the patient, is often very sudden. No sooner does the bile begin to flow, than he *feels* better. Then follows the first bilious stool, and the first passed without tormina or tenesmus. Sometimes it is a jelly-like mass, stained of a dark but vivid olive, or almost black by reflected light. This is the colour of cystic bile—of bile that has become concentrated and darker by retention in the gall-

bladder ; and is evidence, *firstly*, that the liver's function has been suspended ; *secondly*, that it has just been renewed.

Sometimes there is a copious deposit of feculent matter, looking almost black by admixture with this cystic bile ; sometimes there are also scybala that may have lain in the colon for an indefinite time. But, however varying in character the dejections, the correct diagnosis of this colouring matter is of vital importance.

Hundreds have been done to death by its misinterpretation, who even, if left alone, might have done well. For if the practitioner should take these bilious stools, sometimes almost tar-like in colour and consistence, as evidence of "something offensive" in the bowels that must be cleared away, and accordingly should follow up his previous treatment by a vigorous purge, repeated morning after morning, and as long as the stools continue "offensive," he will not only find himself no nearer to, and at length foiled in his object, but he will probably be enabled by the event to add another to the record of those interesting cases of "hepatic abscess in connexion with dysentery," which, owing to the prevalence of less heroic measures, have of late years become comparatively rare. On this subject I would refer to more detailed remarks in the second chapter on bile, p. 49 *et seq.*

Another stage of the disease then, and one stage

of the treatment, is distinctly defined by this first bilious, painless stool, accompanied as it is by all that sense of general relief that marks a crisis past. In the absence of untoward physical conditions—as cold, or erroneous diet, or continued malarious influence—recovery would probably now progress without further aid from medicine. The first bilious stool is succeeded by a moderate diarrhœa of one or two days' duration, which gradually subsides, and at the end of which the patient is rapidly regaining strength, and with an appetite so vigorous as sometimes to need restraint.

But, excepting under very favourable circumstances, it will not be prudent to desist from active treatment. The liver, indeed, is liberated; but exposure to cold, errors in diet, and, above all, the persistence of the primary and local malarious influence (on the absence of which we may never rely), may at any time re-induce a state of acholia, with all its *enteric* results.

Our new indication, then, is to prevent this. The liver is open; and we must keep it so until the equilibrium of health is attained. To sustain the hepatic function, however, when thus awakened, is comparatively an easy matter. We have our courser now well in hand, and we can either goad it on to inflammation and abscess, bring it to a standstill, or entertain a gentle and salutary pace. For the latter object a moderate and timely-repeated

cholagogue is obviously indicated. Small doses of calomel or blue-pill would answer very well; and, were it not for the risk of salivation, there would, in other respects, be no surer and pleasanter remedy. But, as already said, our object is not difficult, and we have ample choice of means: any one of the whole class of emetics and purgatives, judiciously employed, would probably serve our end. Ipecacuanha, in slightly nauseating, but not emetic, doses, is—with those who are in the habit of prescribing it—an excellent remedy. I prefer tartar emetic, as more definite in its therapeutic effects, and also easier of exhibition. I prescribe quarter-grain doses, to be increased, if necessary, within the limits of slight nausea, every eight hours. The stools, that were at first of a bright olive, soon become bright yellow, and the continuance of this lively hue shows that the liver is still under the influence of a sufficient stimulus. A diarrhœa is virtually present, that we have probably the power of prolonging indefinitely. Two days' use, however, of the ipecacuanha or tartar emetic is in most cases sufficient. As soon as the tongue is clean, the skin cool and moist, the urine natural, and the abdomen free from tenderness, we have only to intermit our remedy, and the diarrhœa, *that has been artificially prolonged*, will spontaneously cease.

Next to calomel, tartar emetic is the most efficient cholagogue; and I have treated numerous cases alto-

gether with it, substituting it for calomel at the commencement, and subsequently continuing its modified use in the manner just described. When thus employed in the acute stage—that is, of tenesmus and frequent, scanty stools—it must be exhibited in the way that ipecacuanha has of late years been so much employed—namely, *ad tolerantium*—and will be found, except in certain idiosyncrasies, a more powerful, certain, and manageable instrument. It may be commenced in the proportion of one grain, the subsequent doses being lessened or increased according to the effect.

But ipecacuanha is also a safe and efficacious remedy, and, together with tartar emetic, affords a valuable alternative in those exceptional cases, already referred to, in which the use of mercury is contra-indicated. Of the two, tartar emetic is the more potent; it is more decidedly antiphlogistic; its depressant effect is greater; and it is therefore well adapted for the vigorous and robust, and for those in whom the disease partakes of a sthenic character. In weakly and delicate subjects, and in children, ipecacuanha is to be preferred. The addition of opium under any circumstances is to be avoided. It is used professedly to enable the stomach better to withstand the emetic effect of the bulky ipecacuanha, and doubtless may save the medical officer the trouble of watching the result of his first experimental dose. But when the needed

attention can be rendered, or in a public hospital, where the patient is left in intelligent hands, it can rarely be necessary.

Having considered the direct means of fulfilling our main indication, little remains to be said of auxiliary aids. The following, however, are indispensable to success : confinement to bed, a strictly antiphlogistic regimen during the inflammatory stage, and warmth throughout.

I have rarely had recourse to venesection, but should never hesitate to employ it when decidedly indicated, not only for its speedy effect in unloading the general, and especially the portal, circulation, but as calculated to facilitate our cholagogue intentions. Leeches, however, to the abdomen, followed by warm fomentation and the spongio-piline, constitute a most important part of the auxiliary treatment, and should never be omitted when there is more than ordinary pain and tenesmus. They are sometimes applied near the anus, but are more conveniently applied above, and answer the purpose desired at least as well.

Enemata of any kind, as long as the acutely inflammatory stage lasts, are both painful and perfectly useless. A complication with discharges of blood is not infrequent ; but, notwithstanding these, it must be borne in mind that an absolute constipation no less exists—the bowels inert, and perhaps even loaded with scybala. The inexpediency, there-

fore, not to say absurdity, of exhibiting astringent remedies, calculated to increase this constipation, is evident, and our efforts must still be directed to the removal of the primary source of irritation. With the relief and unloading of the portal vessels, the local hæmorrhage will spontaneously subside.

CHAPTER VI.

CHRONIC DYSENTERY.

ACUTE and chronic dysentery are undoubtedly stages of the same disease. The first is commonly of the duration of several days. Sometimes, however, I believe it may be of a few hours only—that is, counting from the commencement of tenesmus and scanty mucous dejections—for the condition preceeding this again, of acholia (*torpor hepaticus*), may have existed for an indefinite time before.

The shorter the duration of the “acute” stage, the more violent the reaction, and the more dangerous the disease. Many cases do not come under the notice of the physician at all until the acute, or rather primary, stage is over. Sometimes, as already observed, this *constipative* stage—for even when a patient is passing blood and mucus twenty times a day, he is none the less suffering under a veritable constipation—has been so short, or so insidious and comparatively painless, that he does not suspect himself seriously ill until reaction has

set in, and the intestines, from a state of dry congestion, have passed into an opposite of copious and morbid secretion.

Such instances are of so common occurrence, that not a few authors, perplexed by finding that some cases commence with tenesmus and virtual constipation, the patient constantly on the night-chair, and passing nothing but a little blood-stained mucus, whilst other cases are seemingly ushered in with profuse diarrhœa, the patient passing stools more or less bilious, with perhaps an admixture of muco-purulent matter and blood, have inferred the existence of two distinct diseases. But, viewed from a pathological and strictly philosophical point of view, I have no doubt of their identity as stages of the same disorder, nor does any practical convenience attend their separation.

Nothing is more common than to find, in persons who have died from chronic dysentery, the gall-bladder distended with a dark, tarry bile, and at the same time the common duct empty and more or less contracted, as if its natural use had been for some time suspended. And yet for weeks or months before death the motions will have exhibited a variety of hues—ash colour, or clay colour, or every intermediate shade from a dull yellow to a dingy green. An attempt has been made to explain this on the hypothesis that though only a little of this viscid, tar-like bile has escaped into the duodenum,

still that this minute quantity has sufficed to tinge the contents of the bowels. I have elsewhere shown that similar discolorations of the *egesta* have been met with under circumstances where the source of colour could not possibly have been *hepatic* bile. But a yet more fatal objection to this explanation is found in the fact that in the cases under consideration, the intestines, when examined, show more of the colouring matter *below than above*; more throughout the colon than in the ileum, less in the jejunum, less still in the duodenum, and none at all in the ductus choledicus communis. Thus there appears no other way out of the difficulty but to concede that the origin of this colouring matter is within the alimentary canal itself. By doing so we admit a ray of light where all before was obscure and inscrutable; our pathological revelations allow of easy interpretation; and we are enabled to employ our resources, if not more successfully, at least with a more definite conception of the object in view, and of the indications of cure.

The chief of these indications then will be, *firstly*, to restore the function of the liver; *secondly*, to suppress the excessive and vicarious action of the intestines.

But it is imperative that both these indications be carried out contemporaneously. We must never lose sight of the circumstance that this vicarious action of the bowels has, in fact, been instituted

beneficially, to compensate for the want of true bile; and if we interfere to suppress this faetitious secretion without at the same time restoring the true, we shall be holding both hands of nature, and defeat the means that might have prolonged existence for months, or even have resulted in restoration to health.

In a confirmed stage of chronic dysentery, when the amount of enteric bile has so slowly and insensibly increased that its supply has become, as it were, a habit of the system, the action of the liver is not only proportionately reduced, but we may take it for granted that there is a kind of equilibrium established between the two—between the supply of *hepatic* bile on the one hand, and of *enteric* on the other; so that as either diminishes the other increases, and *vice versâ*.

Now, under such circumstances, the indication of cure is evidently to suppress the secretion of enteric bile, and promote that of hepatic—a very nice and difficult matter of attainment, but still of the utmost importance to keep steadily in view. The difficulty, it will be readily understood, arises from the fact that all medicines that act on the liver *may* act as well on the alimentary canal; and the same holds conversely. In a state of health, or even in the early and acute stage of dysentery, whilst the impressibility of the liver is still unimpaired, we may be pretty sure that our large doses of calomel,

or ipceacuanha, or tartar emetic, or other *cholagogue*, will act *electively*, and pre-eminently at least, on the liver. But in prolonged disease these therapeutic relations have become deranged; the intestines have to a certain degree acquired the specific sensibilities peculiar in health to the liver, and habit has become a second nature. Under such altered physiological conditions, our purposed cholagogue may very possibly act altogether on the intestines, thus aggravating the disease, and hastening a fatal termination.

When calomel is administered under such circumstances we may scarcely hope to see the intensely dark green and jelly-like motions that at other times we hail as preliminary tokens of the liver having resumed its vigour and supremacy. The already pale yellow stools, of a faint and sickly odour, may momentarily acquire a darker shade of yellow or green, but not of that vivid hue characteristic of *cystic* or hepatic bile, and will soon again subside into their native dinginess, with an increased sense of weariness and languor to the unfortunate patient.

Could there be found an astringent remedy with which to restrain the bowels, on the one hand, and a pure cholagogue to incite to simultaneous action the liver, on the other, we should probably hold the means of carrying out the required indications. But here the resources of our art fail us; or

rather, the sympathies subsisting between these two great organs and their relative sensibility have been rendered, by chronic disease, so changeful and obscure, as to frustrate the power of applying our remedies with any approach to certainty.

Hence there is no functional disorder of which the treatment is so various, necessarily tentative, and comparatively unsuccessful, as dysentery that has settled down into this chronic form. Not but that even a large majority of such cases may and do recover, but that we are rarely warranted in assuming the whole credit of cure. Change of air, or of diet; or a change in the prevailing wind, or in other conditions of the atmosphere; or in the physical and moral circumstances of the patient himself; one or more of these, seconded by the *vis medicatrix naturæ*, have generally at least as much share in the result as any remedies that have been employed. And should a series of these, as is often the case, have been resorted to in succession, the last prescribed will probably enjoy the repute of special efficacy.

Beyond doubt, the best chance of cure, in obstinate and protracted cases, is to be found in change of air, with all the other changes in the feelings and habits of the patient that this entails. Should the disease be of malarious origin, this course is so obvious as to need no particular illustration.

“Amovendâ causâ, tollitur morbus.” I have known numerous cases of long-standing dysentery, in which the subjects have had to be lifted into the ambulance or litter, and who at the end of the first stage have been strong enough to walk, experiencing at the same time strange and sudden cravings for beefsteaks and bottled stout, and other such instincts of healthy appetite as had not been felt for many a day. Not a medical officer who has served in the East, nor even a purely military veteran, but who can testify to such instances occurring within his own knowledge.

Indeed, this very general efficacy of a change of locality seems to me to furnish one of the strongest arguments in favour of the commonly malarious origin of the disease, although at the same time I am equally confident that, in the entire absence of this specific proximate cause, other and predisposing causes are often in themselves adequate to produce all the objective symptoms of dysentery, in the same way that *sporadic* cholera may be induced in the absence of an epidemic and specific influence. And these cases must, for obvious reasons, constitute a more curable class.

On the other hand, I have known exceptional instances in which change had been tried without benefit, and a return to the original locality has been followed by rapid recovery. A young officer, who was sent home from India, was under treatment

both in England and Scotland, and varied his residence repeatedly during a space of two years, at the end of which time he returned, still suffering from the same disease, to his original station, Trichinopoly, where he immediately got well. I saw him a few months after, and he certainly had no trace whatever of his former symptoms. From an originally slim and remarkably active youth, he had become stout, and even corpulent. He was, however, pale, with a pasty complexion, nervous, and a prey to paroxysms of despondency; had frequent recourse to purgatives; and, though not intemperate, took daily, and with apparent benefit, from two to three bottles of beer. He survived seven or eight years, but I am not acquainted with the immediate cause of his death.

This sudden accumulation of fat is not an unfrequent *sequela* of severe and protracted abdominal disease, especially in India; nature seeming to envelope with a protective layer the permanently enfeebled viscera, anticipative of that later condition, natural only to more advanced age; for in old persons, however constitutionally spare, there is generally some abdominal protuberance, caused by this increase of adipose matter. In them it is to be considered in the light of a beneficial provision; in men who have not reached the prime of life all superfluous fat must be regarded as a comparative token of debility; but when it is met with in the

young, and has rapidly supervened on recovery from fever, dysentery, or other tropical and malarious disease, it possesses a still more unfavourable significance. Under such circumstances the constitution has been in all likelihood prematurely enfeebled, and the assimilative process is carried on in an imperfect and sluggish manner; the organs concerned, and the liver especially, having lost both their native sensibility and their functional elasticity. This constitutional degeneration is often accompanied too by a craving, and perhaps a physical necessity, for stimulants. Such young men are often to be seen in India, conveying to the unpractised observer an outward semblance of lusty strength, but withal pale and listless, incapable of continued exertion, and, in the advent of disease, necessarily easy victims—old, in fact, before their time. It may, however, be easily imagined that in a country where the insurance of life is so universal, such are often accepted by officers as fair average lives, on the ground of a superficial examination, and from ignorance of these indications.

Returning to the treatment of “chronic” dysentery, there appears, as already said, to be a kind of equipoise established between the secretion of the alimentary canal and that of the liver; the excessive action of the one restraining that of the other, and *vice versâ*. A parallel relation subsists between other organs that act in direct sympathy with each

other. Thus, when the perspiration is checked by cold, the excretion of urine is augmented; in diabetes, on the other hand, the skin is dry. In pregnancy again, so long as the puerperal function endures, that of the mammæ remains dormant; no sooner, however, is this at an end, than the secretion of milk as suddenly begins. Moreover, whilst the mammæ thus continue to act, the uterus will not readily take on its proper action again; but should it nevertheless do so, and pregnancy supervene during lactation, again the mammary function is arrested. The idea of an analogous sympathy subsisting between the liver and alimentary canal is surely very interesting, even in the absence of obvious practical application.

Conversely, then, we may sometimes hope that, by checking the excessive and morbid discharge from the bowels, the secretion of bile may be thereby increased, though we know that this is by no means a uniform result. By the employment of direct and powerful astringents—catechu, kino, logwood, opium, and the like—we may indeed temporarily arrest altogether the intestinal discharge; but it is doubtful whether such treatment under such circumstances is often successful. Physiologically it certainly is possible, for the extreme measure of suspending all enteric secretion may, by instituting a crisis of urgency, induce on the part of the liver a paroxysm of effort, resulting in an abundant and

continuous flow of bile. Such an issue is conformable to our experience of nature's ways of working under desperate circumstances. But we should be scarcely justified in trusting to such a chance ; for, in the too probable event of failure, our patient's case would be rendered more than ever deplorable. In ignorant and empirical hands such hazardous treatment is common enough ; the more unjustifiable, since the primary astringent effect is calculated to raise in the sick mind a fallacious prospect of recovery.

The treatment, then, by powerful astringents must meet with unqualified condemnation, and we must endeavour to carry out by other means the *two* capital indications prescribed for our guidance. Seeing that the sudden and total arrest of morbid *enteric* action is not likely to be followed by the other necessary condition of success, increased action of the liver, the next and obvious resource would be the employment of *partial and moderate astringent* only. And judging from my own experience, and from that of others, I believe this to be the best and safest course, at the same time that it is thoroughly consistent with the pathology of the disease here assumed.

By gently and gradually reducing the superabundant flow from the surface of the bowels, the liver is at the same time gradually incited—not to one stupendous and probably futile effort, as when

a powerful astringent is employed—but to a modulated and daily increasing habit of secretion, till at length that healthy equilibrium is attained, when we may withdraw our medicinal aid, and trust to the awakened and now well-directed operations of nature. By this course we not only give time to the liver to resume its supremacy, but we have the advantage of watching from day to day the effects of our remedies, increasing or diminishing them, or substituting others *of the same class*, as occasion may demand.

I have already said that a valid objection to the employment of simple astringents—amongst which I would include catechu, kino, galls, and the like—and also to the employment of opium, consists in the circumstance that, however much their astringency may be exerted on the alimentary surface, this is as much, or still more so, exhibited on the liver; so that whilst we are fulfilling one of our indications, we are making more difficult the other. This opinion seems warranted by the results of their administration in disease generally; but corroborative evidence may be found within the scope of daily observation, in the circumstance that after the employment of any of these vegetable astringents, the *egesta* assume a clay colour; and I need scarcely say that such is conspicuously the case after a dose of opium. From such results we may conclude that however such remedies

may astringe the bowels, they will equally astringe the liver.

But there is another class of astringent remedies, to which the same objection does not appear to apply. From observation of the effect of the various *mineral* astringents, I am strongly impressed with a conviction that, whilst the stomach and intestines are quite as susceptible of their effect in this respect, the liver is much less, or not at all so. Indeed, I am disposed to regard them, on the contrary, as all more or less cholagogue in their action. And if this supposition be correct, we are actually in possession of a whole class of medicines, that enables us to carry out both our capital indications at once; *to constringe the relaxed bowels on the one hand, and to stimulate the liver on the other.*

After a dose of opium, or any of the *vegetable* astringents, as already noticed, the motions will be clay-coloured. But I have never known such a result follow the exhibition of iron, zinc, or copper. On the contrary, in the case of the last two, when exhibited in other derangements, and judging by the brighter aspect of the dejections, the secretion of *hepatic* bile appears to be increased; whilst with respect to nitric and hydrochloric acids, which undoubtedly do sometimes exercise an astringent effect on the bowels, their antagonistic cholagogue action is universally recognised.

These views of the twofold action of mineral

astringents will, I trust, not be deemed altogether unimportant ; for, setting aside the fact of their conformity with my own theory of the pathology of this and other kindred diseases, I would submit that they are in accordance with, and serve to explain, the acknowledged success that has attended the treatment of chronic dysentery by this particular class of remedies.

Acting under the influence of these views, and encouraged by a fair share of success, I have not for many years much varied my treatment of this obscure and insidious disorder. And I am also disposed to conclude that the same general mode of treatment has been found the best in other hands as well, and that the use of the mineral astringents, including the mineral acids, variously combined with tonics and other auxiliaries, is now adopted and approved by a majority of the profession. It remains for me, therefore, only to express my views as to the best mode of availing ourselves of their useful qualities. I shall also take this opportunity of bringing to notice the use of nitric acid in the form of injection.

Another peculiarity of these mineral astringents, and one on which their efficacy in a measure probably depends, consists in their tonic property. So that, when administered under these circumstances, their effects may be characterized as at once *astringent*, *cholagogue*, and *tonic*.

In the ordinary treatment, however, by these

remedies, there is one feature the propriety of which I am disposed to question, and that is their conjunction with opium. If this combination be successful, as it often is undoubtedly, I believe the mineral astringent to be efficacious, not through its aid, but in spite of it. Just as a scruple of calomel acts on the liver in spite of the opium with which it may be clogged. No doubt there may be special circumstances—as, for example, excessive irritability of the system generally, or of the bowels in particular—that might render such an addition advisable. But, under ordinary conditions, I believe the opium to be added with some indefinite notion of limiting the effect. This object, however, might be as well attained by simply reducing the dose, and at the same time without preventing, as opium infallibly does, the action on the liver.

Many combine with zinc or copper not only opium, but a small proportion of calomel. Could we be sure of the calomel stimulating, not the already over-stimulated bowels, but the torpid liver, this no doubt would be a judicious adjunct. But, as before shown, in chronic dysentery calomel does not usually act on the liver; whilst, on the other hand, given in sufficient doses and alone, it increases the irritation of the intestines. If, therefore, when a *smaller* quantity, as half a grain, is added to three grains of the sulphate of copper, a beneficial result ensue, it is attribu-

table only to the preponderance of the stronger element.

I have tried these combinations both with calomel and with opium, and I have tried the mineral astringents alone, with the conclusion that the latter form is at least equally effectual. And, besides this, I need scarcely point out to the experienced practitioner the advantage, in the use of a *single* remedy, in being able to assign results to a distinct and definite cause, and so to shape our future measures accordingly.

Of the mineral astringents, zinc, copper, iron, silver, lead, and bismuth, and of the so-called "mineral" acids, especially nitric and muriatic, I do not think any one entitled beyond the rest to the credit of superior and universal efficacy. In this regard our treatment must still remain in a great measure tentative. Perhaps their relative value depends on some inappreciable conditions of the liver and bowels, and doubtless also on individual peculiarities. I usually commence with a quarter-grain of sulphate of copper (without opium), gradually increasing the dose to gr. iij., or more, and continuing this so long as it seems to act beneficially. Should the results at the end of a few days be equivocal, a trial may be similarly made of the acetate of lead or the sulphate of zinc.

Should these not answer, or, as is often the case, should the benefit at first derived from their

use not continue, it will be well to give the nitric acid a fair trial, and in many cases it proves a most satisfactory remedy. It may be given in doses of from five to ten minims, with the infusion of eusparia, thrice a day. But I have latterly found the best mode of exhibition to be with honey or sugar, made up into a mixture with plain water. It is not only that this constitutes a more agreeable draught, acting less on the teeth, but the saccharine matter seems to counteract the crude activity of the acid, rendering it less likely to disagree with the stomach, without in the least impairing its efficacy. I rarely now prescribe the mineral acids, whether in this or any other disease, without such addition. In accord with this notion of the anti-acid *physiological* effect of sugar, it is worthy of note that acid, and even unripe fruits, as rhubarb and green gooseberries, that would certainly disagree with the stomach alone, are rendered wholesome by a liberal accompaniment of sugar.

Nitric acid not only appears to act on the liver when applied to the assimilative mucous membrane, but also when applied to the skin. In this respect it bears a curious resemblance to the other great cholagogue, mercury. Benefit is often derived from bathing with it the abdomen, the feet, or even the whole body. On this head I cannot do better than refer the reader to Sir Ranald Martin's experience and instructions, especially as pub-

lished in the *Lancet* of the 2nd and 9th December, 1865.

There is, however, yet another use of nitric acid, which I originated some years ago, and which I can strongly recommend in all cases of protracted dysentery. In this disease, when it has lasted for any time, it is, I imagine, very rare for the colon, at least in its lower part, and the rectum, to be free from patches of ulceration. At Secunderabad, twelve years ago, I had a case of old dysentery in which the stools exhibited daily an unusual share of sanguineous purulent matter, in part, at least, the evident *washings of ulcers*. Having also had lately several cases of ulcer, chiefly on the legs, that had become quickly healthy under the use of strong nitric acid, it occurred to me to try it similarly in this case. Two drachms accordingly of the strong acid, with one ounce of water, were injected *per anum*. The following day the patient declared not only that the injection had not given him pain, but that it was followed by a pleasurable sense of glowing warmth throughout the abdomen, and that he had felt much better ever since. It was accordingly repeated daily, and the man rapidly recovered. It was evident in this case, from the general amendment, that the local benefit was the least part of the remedial action, and that in its constitutional effect a result had ensued that I had by no means anticipated. That the injection would

not cause much pain I had predicted, on the simple ground that ulcers of any standing in mucous membranes are generally the reverse of sensitive.

Since then I have tried the nitric acid injections in some hundred cases, both of chronic dysentery and of *acholic* diarrhœa, and with still increasing conviction of its value; so that I now regard it as the easiest, safest, and most speedy means of securing the specific effect of this valuable remedy. It may be used either in the small and concentrated form already described, or as an ordinary enema, in the proportion of $\text{ʒiv.} - \text{ʒvi.}$ of the diluted acid to a pint of warm water or gruel. In either case it is better to commence with a moderate strength, to be gradually increased according to the effect and feeling produced. As a general rule it will be found that the older the disease, and the more conspicuous the signs of ulceration, the greater the strength of the acid that may be borne. For it is probable that in a healthy state of the membranes such a strong application would cause considerable pain. In all cases, therefore, before using the concentrated form, the presence of indolent ulceration should be ascertained by inspection of the stools.

The use of the injection is quite compatible with the contemporaneous exhibition of other remedies, or with that of the acid itself, in the form of draughts.

Under every kind of treatment the observance of a proper diet and regimen, with a carefully regulated temperature, is indispensable. The diet should be light, simple, varied, and nutritious, with the avoidance only of such articles of food as have been found by experience to disagree with the patient. The use of wine or beer must depend on a variety of considerations to be estimated by the medical attendant. The protection of the organs immediately involved from vicissitudes of temperature is of the very greatest consequence. For this object, besides warm clothing, a flannel binder should be worn, but much higher than is the custom, well over the epigastrium, so as to protect the stomach and liver. The parts below are much less susceptible to cold, probably on account of lying deeper, and from the interposition of the bladder.

I have known several cases of chronic dysentery much benefited by "packing" of the upper part of the abdomen on the hydropathic plan.

Some observations on the supposed connexion of hepatic abscess with dysentery have already been made in Chapter II., "On the Nature and Properties of Bile." I will, therefore, here limit myself to the following further remark on this subject.

Because abscess is sometimes found associated with ulceration of the intestines, some have imagined that the former is caused by the latter, and have even endeavoured to account for this by sup-

posing that pus globules find their way through the portal veins from the intestines to the liver. It has also been observed that, though ulceration of the bowels very often exists without hepatic abscess, abscess is never found without some traces of ulceration, which fact has been urged as a proof that abscess is always subsequent to, and a consequence of, ulceration. But the simpler truth appears to me to be this: that functional disorder (and especially inaction) of the liver is a sure and unfailing cause of irritation in the digestive canal, displaying itself in various forms of congestion, as signalized by melaena, hæmorrhoids, and the like; and also in every form of inflammation, including ulceration. But functional disease of the liver is the most frequent cause of abscess as well. So that abscess and ulcerated bowels, instead of standing in the relation of cause and effect, are simply both effects of one common cause. Hepatic inaction (acholia, or *torpor hepaticus*) is *always* accompanied by more or less irritation in the digestive canal; *sometimes*, in certain habits and under certain conditions, it produces abscess *as well*.

CHAPTER VII.

ON CHOLERA: ITS NATURE.

ARGUMENT.—*Cholera is an imponderable matter, or condition of matter; tellurian in its origin; existent in and with the atmosphere, but forming no component part of it, being of it as independent as are the rays of light; attracted by some other matter, or condition of matter, existent in and peculiar to the human body. Where a great space, as a continent or arm of the sea, intervenes, the earth itself may become the conducting medium. But all matter, whether animal, vegetable, or mineral, serves as a conducting medium.*

When present (that is, in the atmosphere, or supra-tellurian) it is attracted, and more or less neutralized, by every, even the healthiest, human body. In certain states, however, of the body, varying from health, the force of attraction is stronger, and the process of neutralization also more active and conspicuous. So long as the vital powers are sufficient to keep up this process of neutralization, and so preserve an equili-

brium, the system is not overcome by the disease. But, under unfavourable conditions, the powers of life are not adequate to carry on, through the lungs, a continuous process of neutralization. The poison is then, of necessity, conveyed through the lungs into the blood. Being there, a far more energetic process for its neutralization is required. A violent convulsion of nature ensues, and the symptoms of "Cholera" are present.

I. In proceeding to consider in detail the above definition of the poison known as Cholera, I need touch but lightly on the preamble, referring to properties that cannot, in the present state of our knowledge, admit of demonstration. Whether cholera be a material poison, ponderable or imponderable, or only a condition or quality of matter, we shall probably never know; nor probably, for purposes of practical utility, would such knowledge much avail. As we can calculate the laws of gravitation without any acquaintance with the nature of that force, and as we can examine the composition and properties of light without knowing whether light be matter, or only a condition of something material, or even of something immaterial, so we may profitably study the origin, progress, laws, and consequences of the cholera poison, without any definite conception, much less any absolute knowledge, of its nature.

I shall proceed then at once to state the grounds for belief in its tellurial origin.

All nature, as known to us, is comprised in the animal, vegetable, and mineral kingdoms ; and, with the exception of solar light and heat, in one of these three we must look for the source of every physical object of research. Cholera poison, then, is either of animal, vegetable, or mineral origin. It may indeed arise from the joint operation of two, or all three, of these ; but for the sake of simplicity, and that such a conjecture is not countenanced by analogy, we will assume its origin in one only.

If, then, it can be shown with respect to *two* of these that a source in either is not to be reconciled with our observation of its progress and effects, and if, on the other hand, no such objection exists to the supposition of its origin in the *third*, but that, on the contrary, such an hypothesis be consistent with what we do know of its rise, progress, and effects, then negative proof of the strongest kind will thus be afforded of the correctness of the *third* supposition. In other words, I propose to show that an *animal* origin is scarcely possible—that a *vegetable* is improbable—and that a *mineral* remains as the alternative, such conclusion, moreover, being consistent, or at least not inconsistent, with known facts.

In India and other tropical countries the wind blows for months together in one constant direc-

tion, cholera often steadily advancing in a course diametrically opposite. This would be impossible if it were a component part, or even a condition, of the air.

Fevers, on the contrary, continued and intermittent, come in with and remain during the prevalence of particular winds. Now we know no more, chemically or tangibly, of the nature of fever poison than we do of cholera ; but we have evidence in this, as well as in many other familiar phenomena, that the former is an atmospheric condition ; so much so, that in a country where fever is endemic, we may predict an immunity from it so long as the wind blows from one quarter. But we can form no such conjecture respecting the movements of cholera, which is just as likely to advance against the wind as with it. Nor, when once established in a locality, is there the slightest ground for supposing that any wind would avail to carry it away.

The fact, then, of cholera not being atmospheric, and of its possessing no properties in common with the class of fever poisons that are undoubtedly atmospheric and of vegetable origin, furnishes a twofold presumption against such an origin for itself.

Again, if it were of vegetable origin, instead of arising at one point of the earth's surface, and then progressing or radiating to others, we should

find it appearing at distant points simultaneously—in Asia, Europe, and America—wherever the necessary *vegetable* conditions existed. For, in the Delta of the Ganges, where the disease originated, and whence it travelled by distinct and progressive stages, there is no reason to suppose the existence of any form or forms of vegetable life peculiar, and not to be found in other parts of the same and adjacent Burmese continents.

But the same objections that apply to the supposition of a vegetable, will apply with even greater weight to that of an animal origin. For if there be no vegetable, much less can there be any animal conditions confined and peculiar to the place of its birth.

Without here anticipating the question of cholera being a contagious disorder, I think it may at least be assumed that it does not possess those decisive features of a contagious malady, such as are displayed, for example, by plague and small-pox. And, if it were of *animal* origin, it might be expected to display that property of contagion to the full as unmistakeably as those two diseases. That it fails to do so furnishes to my mind another and most cogent argument against an animal origin.

I shall scarcely be expected to offer any definite opinion as to the particular geological conditions required for the evolution of the cholera poison. Within the “crust of the earth” unceasing changes

are doubtless in operation—preparatory, perhaps, to the next great epoch in this planet's history; whilst on its surface corresponding changes, slow but unremitting, are taking place in the mutual relations of land and water. One need only compare a modern with an ancient map of Egypt to recognise at a glance the changes in the Delta of the Nile; and similar influences, in operation at the present time, are nowhere more conspicuous than in the Delta of the Ganges, the cradle of cholera.

Cholera, it must be remembered, is not the first pestilence that has appeared on the earth. In all ages since the creation of man, new diseases have thus arisen, culminated, and become extinct. But there is no reason to suppose that, within that same "historic" period, any corresponding changes have taken place either in the animal or vegetable systems. In the geological arrangement, however, both above and below the surface, change has been, as already observed, incessant; and there is nothing improbable in the supposition that, in the course of these terrestrial operations, influences hostile to animal life may from time to time have been evolved.

The following, then, may be summed up as the chief arguments in favour of a "tellurial" origin of cholera:—

Firstly, the absence of the properties common to

that class of diseases of which ague is the type, and which are undoubtedly of vegetable origin.

Secondly, the absence of an obvious contagious property, such as displayed by other diseases undoubtedly contagious, and which contagious property could not well be absent or dubious if the poison were of animal origin.

Thirdly, its not material nature, as proved by its independence of the movements of the air, which nature would strongly indicate a tellurian or mineral origin.

Fourthly, the absence of any known facts in the history of its rise, progress, or effects, opposed to, or irreconcilable with, a tellurian origin.

Fifthly, its strikingly partial distribution. With respect to this last character, in itself almost conclusive, as it seems to me, of tellurian relation, Dr. Baly writes in his Report, page 9,—“Again, if the attention be fixed on single towns, single streets, or public institutions, the same character is observed. One part of a town suffers most severely, others escape altogether. In a few houses in a street half the inhabitants are carried off by the disease ; in the remainder not a single death occurs. In a large public establishment, such as a barrack, a lunatic asylum, or a prison, it often happens that the disease is, at least for a time, confined to one wing of the building, one ward, or one series of rooms. (See instances in the Appendix of Report.)”

Before abandoning this part of the subject, I would direct attention to a summary given below of the history and progress of cholera, on the first two occasions of its visiting this country, also extracted from Dr. Baly's admirable Report. It will be observed that, starting from Bengal in 1817, it travelled, sometimes rapidly, sometimes slowly, at times appearing even to reeede; now in a direct line, now by advances more or less tortuous or eccentric, but still ever onward and progressive, until, *after journeying for fourteen years*, it reached these shores. A second time, seventeen years later, we find it on the very same track, tending in the same direction, and travelling at the same pace. And now, for the third time, and again after an interval of *seventeen* years, we are engaged in watching the movements of this mysterious foe—with a gloomy prescience, the result of past experience, that it is coming; moreover, with a tacit, but settled and universal, conviction that no human means, no measures of quarantine or purification, will avail to stay its course. Would any such certainty attach to the progress of an intermittent fever, or of any kindred disease reputedly of *vegetable* origin; or, were it the plague, or other pestilence confessedly contagious (and presumably of *animal* origin), should we not have been busy with expedients for limiting the *media* of conduction? How is it that the common sense of Europe, in despite of the instigations of a

very small section of physiologists, thus rejects the notion of quarantine, or of any other *obstructive* or anti-contagious measure whatever, relying rather on those recognised principles of sanitary science that have for their object the attainment of such a standard of public health as will best enable each community to withstand the evil when it comes? What is it but the conviction prevailing among all classes that we have to deal with a nature infinitely more subtle than that of plague, small-pox, or typhus, whilst the scientific mind, from a parallel point of view, thinks to recognise a nearer affinity to things imponderable and immaterial, such as magnetism, than to any conceivable class of *material* poisons, whether animal or vegetable?

The following is the summary referred to:—

“ The first fact to be noticed in the more general history of epidemic cholera is its progressive march. This is obvious in the dates of its first outbreaks in the different countries lying between the Delta of the Ganges, where it originated, and this country. Having spread over the valley of the Lower Ganges in 1817, it passed in the summer of 1818 across the northern part of the peninsula, and reached Bombay in August or September of that year. It was not till July, 1821, that it appeared at Muscat, at the mouth of the Persian Gulf; but in September of the same year it was at Bagdad. It extended through many parts of Persia and the eastern pro-

vinees of Turkey in 1822. In 1823 it passed the Caspian Sea, and in the month of September showed itself at Astræan. It made no further progress, however, in Europe until the year 1830. In that year, having appeared again at Astræan in June or July, it extended rapidly through the eastern part of Europe, reaching Moseow in September. In the following year it extended to Riga and Dantzic in May, then to St. Petersburg in June, to Berlin in August, and to Sunderland, in this country, in November. So again, in the later invasion of Europe, starting from Astræan, where it appeared in June, 1847, it was felt at Moseow in September of the same year; at Berlin, in June, 1848; at Hamburgh, in August; in London, at the end of September; and at Belfast, in Ireland, in December."

II. The next matter for inquiry is the proposition that this "imponderable matter, or condition of matter (cholera), is attracted by some other matter, or condition of matter, existent in, and peculiar to, the human body."

The idea of such a special attraction has been forcibly present to my mind ever since I have enjoyed the opportunity of observing the movements of cholera in the East. Whether it be a caravan crossing the desert, or a concourse of pilgrims to Mecca, or a stream of devotees crowding to some great Hindoo festival, or a Madras regiment with

its train of ten or twelve thousand followers, all these masses of moving human life seem to exercise a powerful attraction on the cholera poison ; and this attraction must extend to great distances, for it is notorious that a regiment in India on a march will contract and carry along with it cholera in a district where the epidemic is at the time unknown. Dr. Baly, in his Report, observes, at page 135 :—“ Cholera has frequently attached itself to bodies of troops on their march in India, and has remained with them during many days in their passage over long tracts of country, the inhabitants of which were not suffering from the epidemic. The duration of the epidemic in marching regiments is stated by Dr. Lorimer, who has collected the largest number of instances, to be in the majority of cases (in 88 out of 121) less than thirty days. It has often, however, been longer, and in the more severe outbreaks the epidemic reached its climax in the regiment about the eighteenth day. Many points relating to these attacks of marching troops in India are matter of dispute. But the fact that the disease remained with them many days, *when it did not prevail in the country around*, seems not to be gainsaid ; and here, again, the inference is irresistible, that the cause of the disease travelled with the troops.”

But, whether in Asia or in Europe, if we attentively consider the movements of cholera, I think

we shall find that, for its dissemination, there is one dominant, invariable, and indispensable condition. If it seem to follow the course of large rivers, as the Ganges, the Danube, or the Mississippi, we find that these do but represent so many tracks where human life exists in the densest masses. Or again, in its predilection for seaports, for places bordering the estuaries of large rivers, or for others placed at a low and marshy level, the same condition is inevitably to be found in a population not only in general more numerous, but always more *crowded* than in places inland, and at a high level. For it must be borne in mind that it is not so much the *size* of a population as its *density*, that seems to determine the power of attraction.

Nor would the circumstance that crowded populations are not always attacked, even with the disadvantage of a site presumably unfavourable, furnish any real ground for an opposite conclusion. On the contrary, such exceptional instances would rather imply the absence of some other conditions, *probably tellurial*, and with which we are unacquainted. Many populous places, both in Europe and Asia, have always escaped, though the pestilence has been repeatedly in the vicinity of some of these, and though apparently possessing the worst possible sanitary conditions, in respect of a crowded population, poverty, filth, and a low level. Regiments in India, carrying cholera with them, sud-

denly part with it on crossing a river, and though their subsequent route be along the opposite bank. By an order of Government it is directed that, when a marching regiment is attacked by cholera, it shall, if practicable, be divided into wings, to continue their march by two different routes : under such circumstances one wing will often carry the disease, the other being at once relieved of it ; and yet there may be no cholera on either road. Such instances are familiar to every Indian official, and, with the others cited above, scarcely admit of explanation, except on the supposition of, on the one hand, a special attraction by *masses* of human life for cholera, and, on the other, the necessary conjoint existence of some *tellurial* conditions unknown.

III. “ *Where a great space, as a continent or arm of the sea, intervenes, the earth itself may become the conducting medium. But all matter, whether animal, vegetable, or mineral, serves as a conducting medium.*”

A tellurial origin would almost seem to involve the corollary of “ tellurial ” conduction ; and this is also naturally included in the general proposition that “ all matter may serve for conduction.” It is worthy of note that on the truth of this general proposition at least one important *practical* question depends.

If, for instance, in the absence of any other more speedy, the earth itself becomes a conducting medium, it follows that measures of quarantine, or other severance of communication, may possibly avail to *delay* the progress of cholera in any given direction, but cannot permanently prevent it. And this seems to accord with, and account for, its slow and fitful, but in the end no less inevitable, advance in those countries of Europe where quarantine has been enforced.

This conducting power on the part of all matter would also establish another characteristic distinction between cholera and the group of "malarious" diseases, as well as between it and those of the "contagious" kind. For the former, as ague, are undoubtedly conveyed by the currents of the atmosphere, and are incapable of propagation in an opposite direction ; whereas cholera—though probably admitting of propagation by means of the air, as of all other matter—undoubtedly possesses the power of advancing "in the teeth" of a strong and continuous wind, a feature eminently distinctive, and in itself strongly suggestive of telluric conduction. On the other hand, abundant experience has proved that, by stringent measures for that purpose, the isolation of a really *contagious* disorder, as plague or small-pox, may be absolutely effected ; but with respect to cholera, experience, little less universal, is opposed to the supposition that any measures of

isolation will suffice to confine it, or stay its determinate advance; another distinctive feature, and equally unaccountable, excepting on the hypothesis assumed of "telluric" conduction.

The practical deduction is evidently this—that if tellurial conduction be a fact, no human means can avail to prevent, permanently, the visitation of a country by cholera. If, on the contrary, there be no grounds for belief in such a medium, and if men and ships be indeed the only means for its conveyance, the neglect of all measures of precaution, on the part especially of an insular nation, does seem somewhat inconsistent. But, as before observed, I believe there prevails, among mankind generally, a deep-rooted conviction in the inadequacy of any such measures, without probably, at the same time, a perception of the fact that such a belief almost necessarily implies—namely, that of terrestrial conduction.

IV. "*When present (that is, in the atmosphere, or supra-tellurial), it is attracted, and more or less neutralized, by every, even the healthiest, human body. In certain states, however, of the body, varying from health, the force of attraction is stronger, and the process of neutralization also more active and conspicuous. So long as the vital powers are sufficient to keep up this process of neutralization, and so preserve an equilibrium, the system is not overcome by*

the disease. But, under unfavourable conditions, the powers of life are not adequate to carry on, through the lungs, a continuous process of neutralization. The poison is then, of necessity, conveyed, through the lungs, into the blood. Being there, a far more energetic process for its neutralization is required. A violent convulsion of nature ensues, and the symptoms of cholera are present."

Already I have endeavoured to trace certain features of distinction between cholera and ague, on the one hand, together with the class of diseases which ague may be taken to represent; and, on the other hand, between cholera and small-pox, together with kindred disorders of the "contagious" type. I have now to point out another character, no less distinctive, and peculiar to the disease under consideration.

In England the visitations of cholera have been happily few, and at long intervals. But in India—at least since its appearance as an epidemic in 1817—not a year has passed in which its ravages have not been repeated, in varying degrees of severity, in the densely populated great cities of that country; so that, whatever may have been the case before, it may now be there regarded as endemic. Many medical officers in India have thus opportunities, annually recurring, of personally observing the features of this disease, and of comparing the observations of one year with those of another. To them,

therefore, more particularly I would refer for corroboration of my own experience in reference to the following fact; namely, that during every visit of cholera to a great and populous city—as Hyderabad in the Deccan, for example, where I had the opportunity of observing the disease during several different years—there prevails, as a constant accompaniment, *a sensitive condition of the bowels*; manifesting itself not only in distinct cases of diarrhœa of every degree of severity, so as, in their severest form, not to be definitely diagnosed from cholera; but, moreover, in a tendency to irregularity and derangement of the digestive function, more or less observable in every individual of a community.

Something similar has evidently been recognised on each occasion of its visit to this country. In Dr. Gull's Report, based on accumulated evidence, and eminently able and impartial in its deductions, at page 124, occur the following noteworthy passages:—

“There is evidently in some minds a disinclination to regard any case as cholera, unless it manifests symptoms of marked intensity; but that the diarrhœa which prevails when cholera is epidemic is due to the same cause as cholera itself, is to be inferred not only from its clinical history, but also from other circumstances.”

“The extent to which diarrhœa prevails, and its

gradations towards cholera, are greatly in favour of such a conclusion."

"Again, its invariable occurrence with cholera apart from any influence of season—for whether cholera prevails in winter or summer, epidemic diarrhœa is attendant upon it, as a shadow upon substance, and is apparently as independent of these external conditions as cholera itself."

It was the observance of this constant attendance of diarrhœa on cholera, as of "shadow on substance," that led me, in the first place, to conclude that they were attributable to the same cause; and next, to suspect that in this relationship might be traced the true *modus operandi* on the system of the cholera poison.

When, therefore, cholera is present with us, I would assume that every human being within the area of its influence is at once brought under, and constantly remains subjected to, its operation; that between it and each living organism there is being waged a silent, but never interrupted, conflict. Perhaps, as already suggested, a gradual neutralization of the "poison" is thus being carried on; but on this point more is to be said hereafter.

A healthy condition of the body, and especially of the digestive organs, renders this counteraction in the great majority of cases an easy, generally an insensible, process. Certain other, even morbid, conditions of the system may also be antagonistic.

In some organisms, however, the struggle is rendered more apparent by sensations of discomfort, passing abdominal twinges, a tendency either to constipation or diarrhœa, generally the latter, and a vague sense of danger.

Sometimes, again, the conflict waxes stronger still. Nature becomes thoroughly alarmed, and preparations are made for a more strenuous resistance. The blood is summoned from the surface to the heart and large vessels; congestion of the chylipoietic viscera ensues, and conspicuously of the portal system, from its remotest tributaries in the intestinal tube to its grand confluence in the liver; a simultaneous suspension of the hepatic function occurs, so brief perhaps as not to be externally recognised, and to be immediately succeeded by a sudden and excessive outpouring of bile. It is possible that, in this energetic action of the liver, the intestines may themselves *vicariously* assist, thus contributing to the relief of the distended and hyper-cholericized portal vessels. Thus much is certain—that by their mutual co-operation a form of diarrhœa is established which in the greater number of cases is sufficient to restore an equilibrium of health, apparently by neutralizing and eliminating from the system the poisonous element. Such I believe to constitute the history of those instances of *bilious* diarrhœa, always more or less prevalent during an invasion of cholera.

The distinction between such cases and those of actual cholera, in which the disease has more completely succeeded in overpowering the resistance of the system, is to be found in the conspicuous suppression of the hepatic and renal functions. The entire absence of bile in the stools and of urine in the bladder are, in their conjunction, truly pathognomic. A fuller consideration, however, of the symptoms and pathology of cholera must be reserved for another chapter.

V. If cholera be attracted by masses of human life with an intensity proportioned to the size and density of those masses, it is not difficult further to conceive that the largest and densest masses will also attract the poison in *amounts* proportionately large. In Dr. Baly's Report the following interesting data are given:—"In 41 registration sub-districts, of which the population was under 10,000, the average duration of the epidemic was 81 days; in 44 sub-districts, which contained from 10,000 to 20,000 inhabitants, the disease continued to prevail on the average 108 days; and in 35 sub-districts, or districts comprising large towns, the population of each of which exceeded 20,000, the disease lasted 117 days. The contrast with these larger places would be more strongly seen in single villages or small towns, several of which are often included in one sub-district." That a larger

amount of cholera poison should be attracted by a large than a smaller population seems, indeed, almost a matter of course, and is illustrated not only by the comparative durations, as given above, but by its greater ravages during those durations.

And if a process of neutralization between the poison and each individual be constantly going on, it is also natural to suppose that, in course of time, that process will in each be completed; and that, when completed in all, the disease will not only be at an end in that locality, but that there will not be found there the same pre-existent power of attraction.

This theory of "neutralization" appears, further, to be strongly indicated by the general history and progress of cholera, but especially by the following well-known characteristics:—

Firstly.—The concomitant diarrhœa, with the phenomenon already adverted to, that, during the prevalence in a town or district of cholera, every human being within its influence seems morbidly affected by it, more or less.

Secondly.—The evident tendency that the disease has to exhaust itself, after a certain amount of *action*, generally proportioned to the size and density of the population.

Thirdly.—The gradual declension in the degree of its virulence, the first cases being always the worst, and generally fatal; whilst succeeding

eases, towards the close of an invasion, become less and less severe.

Fourthly.—The neutralization, and subsequent elimination by the bowels, of a *neutral and innocuous* matter, in seeming accordance with the non-contagious quality of the disease.

From a formal demonstration of this non-contagious character of cholera I refrain, because it would entail a lengthened, and, to most of my readers, a needless disquisition; because it seems to me a matter not more for scientific inquiry than for common experience and observation; and lastly, and chiefly, because it is recognised by a vast majority of the profession, and endorsed by the universal sense of mankind. I will content myself, therefore, with protesting against the inexpediency, not to use any stronger term, of instilling into the public mind a doctrine so mischievous, and that can be, at the best, but of very partial or doubtful truth—a doctrine calculated to intensify to the utmost the terror with which the disease is already regarded; to increase, by its powerfully depressant effect, the liability to attack of every human being; and to deprive the stricken victims themselves of that aid and sympathy that have hitherto been so fearlessly and nobly accorded.

VI. It follows, from what has been already observed, that the absence of a single *case* of

cholera in any community furnishes negative evidence only of the absence from the locality of the poison itself; for, under favourable conditions, the process of neutralization may be carried on and completed without a single individual succumbing. In some of our largest towns solitary cases occurred; in others, two only, or three. Such results may have been due to a less intensity of the poison, or to a general bodily condition more adapted for its neutralization, or to a conjunction of these causes.

Nor does it certainly follow that weak and delicate persons are more liable to be attacked than the strong and robust; nor even unhealthy individuals, in the sense usually conveyed by the term—as, for example, of the strumous, rheumatic, or calculous diatheses. It would rather appear that there is some peculiar and unknown condition of the system that predisposes to attack, possibly even a condition not offering any obvious morbid indication.

But within my own experience—and I think I shall be supported by most who have seen much of the disease—there are, in every community, some few who may be pointed out as more likely, in the event of an outbreak, to be attacked. Such persons, too, seem to have an instinctive sense of their own comparative liability. They are those who, at all times, display an unusual solicitude

about their diet, studying to discriminate between what is wholesome and the reverse. A general susceptibility of the digestive organs does probably, *cæteris paribus*, predispose to cholera ; but I think, in such cases, the depressing effect of fear has a far greater share in the result. A serious, and often difficult, duty hence devolves on the medical adviser in counteracting this morbid and dangerous state of feeling. Such persons should be instructed (in the absence, of course, of actual disease) that safety is not to be found in the selection or avoidance of particular articles of food ; nor in the injurious practice of artificially opening the bowels, when inactive perhaps, and possibly with benefit, for only a day ; nor in recourse to spoonfuls of brandy, on every occasion of fancied or anticipated indigestion. A simple diet, comprising both animal and vegetable food, the proportions of which are best regulated by the instinctive promptings of appetite ; exercise within the limits of fatigue ; the avoidance of every form of excess ; moderation, in fact, in all things—*semper in mediis* ; above all, perhaps, a trustful and hopeful spirit ; these comprehend the best elements of safety.

VII. Dampness, a low level, and other insalubrious conditions, have been assumed to favour the development, and to increase the virulence, of the

cholera poison. It seems to me more simple, as well as more rational, to suppose that cholera commits greater havoc in a population predisposed to disease by a low standard of general health. An inferior site, dampness, and all other unwholesome influences, may indeed thus be regarded as *remote* causes, but there would not therefore necessarily exist a direct and logical relation. The cholera "poison," therefore, is not augmented in quantity, or increased in intensity, by damp, a low situation, or any other unwholesome conditions whatever; but wherever such conditions are to be found, there also will be found a population more or less cachectic or enfeebled, and there naturally will cholera find the greatest number of victims.

CHAPTER VIII.

ON CHOLERA: ITS SYMPTOMS AND TREATMENT.

THOUGH, in the chapter preceding, I have particularly dwelt on the assumption that cholera is not an atmospheric condition, it is evident that a poison of so subtle a nature must possess at least an equal power, with the atmosphere, of pervading space. Thus, though independent, it may be everywhere co-existent. That, therefore, with the air, it should enter the lungs, may be assumed as certain.

It further follows that as, in the lungs, the whole volume of the blood is brought into contact with the atmospheric gases; so too it must be brought into a contact, at least as close and immediate, with this far more subtle entity.

Nor, anywhere else in the human body, can a like contact obtain.

From these considerations it would appear not only that the cholera poison must act, primarily, on or through the lungs; but, inasmuch as the blood can nowhere else be brought into such intimate

contact with a circumambient poison, that such operation can nowhere else take place, and must therefore be entirely restricted to the lungs.

The same reasoning of course will apply, with some modification to suit *special* properties, to all poisons, whose subtle and buoyant nature enables them to enter the lungs in the act of respiration.

One of the most striking and constant results of cholera poisoning is to be found in the icy coldness of the breath, and in the diminution of the amount of carbonic acid respired—with, of course, a corresponding increase of the oxygen accompanying.

Such a sudden reduction in the functional activity of the lungs would be in itself perhaps sufficient to account for, and give rise to, all the succeeding phenomena. That is, if we could, by any *other* poison, or by any other means, induce a similar, and *equally sudden*, effect in the respiratory function, and confined to it, it is not unlikely, I think, that we should also induce *secondary* results, analogous to, if not identical with, the symptoms of cholera.

For what, in cholera, are the more immediate consequences of this sudden defect in the respiratory function? They are precisely what a physiologist might have predicted; firstly, in consequence of insufficient oxygenation, the substitution of venous for arterial blood; secondly, in consequence of insuffi-

cient pulmonary "combustion," a diminution in the aggregate warmth of the body; and thirdly, as an effect of both these causes in conjunction, a great depression of the nervous system.

Now, proceeding another step in our analysis, suppose we could, by any other means than cholera, produce, and suddenly, this *second* order of conditions; that is, venous in place of arterial blood—reduction of temperature—brain and nerve depression. What, physiologically, might be expected to follow?

Why, if in sufficient intensity, speedy death—without the intervention of any other obvious symptoms whatever. Nature might be so paralysed, so suddenly overcome, as to be incapable of any effort of resistance.

And this exactly accords with what is witnessed in the worst cases of cholera. There is no vomiting, and no purging. A soldier is seen to stagger suddenly, and resign his musket to a comrade; in a few minutes he is livid, cold, pulseless. Amongst the natives of India especially such cases are common.

But, again, let us suppose the existence of the *three* morbid conditions specified, but in a minor degree of intensity, and induced (it will be understood) by some other agency than cholera. What resulting phenomena might be expected to ensue?

A physiologist, familiar with Nature's ways, and

reasoning by analogy, might be supposed to predicate as follows :—

1. From the defective oxygenation of the blood, and its consequently impaired quality, more of it would be needed for the use of every part. It would be therefore rallied, in excess, to some, or all, of the internal, and more vital, organs ; leaving the surface proportionately bloodless. Hence would ensue *congestion* of some, or all, of the large viscera.

2. In the same way, from the diminished aggregate amount of caloric, more would be needed for the conservation of these more vital parts. Hence a concentration of heat internally, with a corresponding diminution externally. We might have therefore an internal sensation of great and unendurable heat, contrasted with extreme cold of the surface.

3. From the influence of the third-named condition (loss of nerve-power), in conjunction with the first (unoxxygenated blood), we might also expect suppression of some, or all, of the secretions ; more particularly, perhaps, of those that are the product of glandular structures, specially organized for special secretions, and that are supplied by the ganglionic or sympathetic system ; such would be the biliary, pancreatic, salivary, and urinary.

4. Further, however, it seems to be a general law of the economy that, whenever any one secretion is suspended, there shall be a tendency to the establishment elsewhere of a vicarious substitute. The suspension, moreover, of an important secretion seems inevitably to induce a sympathetic irritation in some one other organ, and in that one pre-eminently. The interruption of the biliary function in particular is found to produce irritation of the alimentary canal, manifesting itself in the form of diarrhœa, or sometimes of dysentery. A more complete suspension therefore of the vital action of this great organ, in conjunction, and simultaneous with a like torpor of the pancreas and kidneys, might be expected to produce a still more sudden and violent effect on the intestinal tube. Hence might be predicted an excessive outpouring of fluid from its mucous surface in its whole extent; in other words, a profuse and watery flux.

5. Lastly, from the well-known sympathy subsisting between the kidneys and the skin, an augmented secretion from the latter would be among the probable results. The surface of the body would be bedewed with a cold perspiration.

In figuring the above pathological consequences of an hypothetieal poison (not cholera), I must trust to the candour of the reader to judge whether

my deductions have been either strained, or inconsistent with our physiological knowledge. As elsewhere observed (page 19), the *order* in which vital actions succeed one another can never be exactly defined; for, in beings so highly organized, these processes are ever mutually acting, and being reacted upon; causes taking the place of effects, and effects of causes; with an endless and ever-shifting intricacy. Ours is not an exact science. The history, therefore, of a chain of morbid events can never be more than an approximation to the truth.

The actual results, then, of the cholera poison will have been recognised as identical with the hypothetical results of a hypothetical poison assumed. They may be recapitulated as follows, in their apparent order of succession.

In the first place, when the lungs have no longer the power of resisting, presumably by neutralization, the poison of cholera, that poison produces, through the lungs, a threefold and simultaneous effect; insufficient oxygenation—loss of temperature—great nervous depression. The patient feels suddenly faint and giddy; the skin becomes deadly cold, clammy, and wrinkled; with a universal leaden tinge, especially of the lips and conjunctivæ. The breath issues cold from the mouth. The tongue too is cold, and of the same general leaden hue. If a vein or artery be opened, even at this early stage, the blood trickles slowly, thick and dark. The

pulse is scarcely perceptible. The shock to the whole nervous system is of the most intense and evident character.

Next in order ensue the more proper, or pathognomic, symptoms of the disease; and if I were required to state, as succinctly as possible, in what these consisted, I should say, "In the total suppression of some secretions, and the excess of others." In consequence of the venous blood in the arteries; of the failing of the natural source of heat in the lungs; and, above all perhaps, of the extreme depression of the nervous system; the secreting powers of all the *glandular* organs, including the liver, kidneys, pancreas, and salivary glands, are absolutely arrested. There is no bile; no urine; and the tongue "cleaves to the roof of the mouth." At the same time, whilst the surface and extremities are cold and bloodless, the viscera generally become hot and gorged with blood. All the heat remaining, and all the blood, are rallied to the strongholds of the body. They are wanted there for present life, and for the death-struggle at hand—for reaction. The patient, whilst his very nails are blue with cold, complains of intolerable heat, especially about the epigastrium.*

* This great internal heat, accompanied by proportionate cold externally, seems to me to account for the unusual warmth developed on the surface of the body after death from cholera. The unequal distribution of temperature during life, which is probably a part of Nature's curative process, can of course be only sustained in a living

With a failing circulation, irregular muscular action is a familiar physiological effect. There are severe and excruciating cramps.

That the sudden and total, *and above all conjoint*, suppression of secretions so important as those of the liver and kidneys, should be attended by a corresponding increase in the normal amount of some other fluids of the body, is also in perfect accord with what is observable in other morbid conditions. In some forms of diarrhœa, and in dysentery, we have an enteric flux, in conjunction with hepatic congestion and acholia (suppression or diminution of bile). On the other hand, in ague; under the influence of terror; from the effects of serpent bites; under all conditions indeed of great depression of the vital powers; we have also more or less visceral congestion, accompanied with perspiration. In cholera therefore (in which both the visceral congestion, and the suppression of important secretions, are of a most complete and unparalleled nature), that we should have *both* these consequences in extremest intensity, is precisely what, physiologically, might have been expected. Accordingly, the skin is bedewed with a cold sweat; and there are frequent and copious dejections of a thin

body. As soon, therefore, as vitality is at an end, the physical laws of heat, no longer controlled by vital agency, operate to induce a uniform temperature; and thus the heat, before collected within, becomes generally diffused, and sensible externally.

fluid, supplied from the mucous surface of the bowels.

There is also, in proportion generally to the purging, troublesome and incessant vomiting, first of the contents of the stomach, and then of a fluid possessing much the same aspect and character as that passed by stool. This vomiting is, in fact, an indication of the extension of the intestinal irritability to the stomach itself, and also of an analogous morbid secretion from that part.

I cannot help noticing here an interesting resemblance that has always appeared to me to exist between the poison of cholera, and that (also, be it observed, introduced into the blood) by the bites of venomous serpents. There is the same sudden prostration of the powers of life, with faintness, sickness, giddiness ; the same characteristic stupor, lethargy, and indifference ; vomiting, mortal coldness, clammy perspiration, livid hue of skin, faltering pulse, and viscid tarry blood. In fact, if we could select those rarer cases of cholera, in which the victim is suddenly stricken, and as rapidly sinks, without either purging or cramps, it would not be easy, I apprehend, to define clearly in what the diagnosis should consist.

And, when we come to examine the pathological conditions, the coincidence in them appears no less striking and complete. There is the same congestion of the viscera generally ; concentration of heat

about the internal parts, with proportionate cold of the surface; suspension of the hepatic and renal functions.

By the way, this suppression of bile, after snake-bites, is sometimes curiously exhibited in the super-vention of jaundice. The same phenomenon, as is well known, will sometimes ensue after a paroxysm of mental emotion. The *abrupt* interruption of the hepatic function is without doubt the proximate cause of this vicarious process. That jaundice does not commonly occur in cholera may possibly be owing to the sufficient removal, by the bowels, of the chemical components, remote and uncombined, of bile. Or, this vicarious and inordinate action of the intestines may be incompatible with the vicarious and simultaneous excretion, elsewhere, of bile. But our knowledge of the sympathies established in the economy, through the medium of the nervous system, is far too limited to admit of anything beyond the merest conjecture on such a question.

In the early part of this chapter an attempt has been made to trace the probable consequences of an *hypothetical* poison, analogous to cholera. The reasoning adopted for that purpose may perhaps be admitted to receive further confirmation, as well as illustration, from this *actual* existence of a class of poisons, bearing, in their physiological effects, a resemblance to the "poison" of cholera, too marked to be altogether imaginary, or merely accidental.

It only remains to be noted in addition, that even the stagnation of the circulation, whilst the work of elimination is going on, is another indispensable element of the defensive process. So long as any of that subtle poison pervades the blood, this cannot flow too slowly ; the more quickly it passes through the parts it supplies, the more, it is evident, must they be subjected to the injurious influence it contains. The machinery of life cannot go on beneficially till this be expelled. It is therefore all but suspended ; carried on at the lowest possible rate compatible with life.

Thus I have endeavoured to trace, in their pathological sequence, as well as in their mutual relations, the symptoms of cholera, as more commonly met with. For a more minute inquiry as to the mode in which a liver congested and torpid, together with a portal circulation congested and hyper-cholerized, unite in producing various forms of intestinal irritation, I must refer the reader to Chapters I. and II. on Bile and the Biliary Function. This will enable us to proceed at once with the further progress of the symptoms under consideration, until their termination in death or recovery.

That symptoms are the results of morbid action, and at the same time *processes of restoration*, is a truth that should never be lost sight of in the study of disease. In all its stages—in every symptom, even to those last that announce the near approach

of death—may still be recognised the efforts, however feeble and unavailing, of Nature. She never yields. But it seems to me that there is no disease in which her purposes are so clear and unmistakeable as in cholera. For there is not one, of which the course is so short, so rapid, so violent ; and, withal, so destitute of *structural* change. It is all, if I may say so, a *violent functional convulsion* ; soon over, and decisive for life or death. Within a few hours after his encounter with this most mortal peril, a man may be seen standing in the street, as well, functionally and organically, as before he was attacked.

Though not always evident, at least as an objective symptom, this flux may nevertheless be regarded as a constant and characteristic feature of the disease. For in those severer cases, cited above, in which no purging precedes dissolution, the peculiar ricewatery exudation is still to be recognised on examination after death. Hence it has naturally come to be considered the most prominent and important symptom of cholera. To the attentive observer it affords an index of the *degree* of vital reaction ; of the amount, in other words, of physical energy available and expended in combating the disease. Of course, on the one hand, the morbid action set up may be too potent to be counteracted by any amount of reaction ; and on the other hand, as already observed, the most rapidly fatal cases are

those in which there is little, or no outward evidence of reaction.

Whether this outpouring by the bowels be an elimination from the system of the cholera poison itself, is a question that cannot be finally decided, until we possess some tangible knowledge of that poison, and the means of testing its presenee. I incline to the opinion that the cholera poison is neutralized in its course through the blood, and that the *product* alone of that neutralization, bland and harmless, is to be found in the fluids ejected. This opinion I ground, firstly, on the entire absenee of any facts, as yet adduced, to show that the evacuations possess any *special* noxious property ; secondly, on the record of a variety of experiments, as well as on more familiar observations, all tending to an opposite conclusion. Of these the following passages from Dr. Gull's Report, at page 121, afford a view both so concise and comprehensive, that I cannot do better than give it in his own words.

“The fluid effused from the intestinal mucous membrane has by some been regarded as a *materies morbi*, possessing the power of propagating the disease. As it is the effect of the morbid action, it is so far a *materies morbi*, but beyond this we cannot at present predicate anything concerning it.”

“ Dr. Snow has framed a theory of the diffusion

of cholera upon the hypothesis of a contamination of the water-supply of cities and towns by the evacuations, but he gives no facts to prove that they have the power he attributes to them, nor have we any evidence that they can excite the disease."

"We can confirm Schmidt's remarks, that those who were occupied in examining the discharges, and inhaled the effluvia from them, felt no ill effects. They were also brought into contact with abraded surfaces with impunity."

"They who were engaged in making post-mortem examinations of cholera subjects seemed to incur no risk of thereby taking the disease."

"Schmidt states, that within his own knowledge, a drunken man by mistake swallowed half a beer-glass of the vomited matters, slept away his drunken fit, and remained well. Many similar facts, he says, were made known during the epidemic of 1831-32, when medical men, by way of experiment, swallowed these transudations without injury; but he adds, 'I have not had self-denial enough to institute these experiments on myself.' Mr. Marshall remarks, that the performances here alluded to were limited to *tasting* the *vomited matters* only, and did not extend to swallowing the alvine evacuations."

"Many experiments have been made upon animals with the blood and effused fluids in cholera,

with the view of determining whether the disease is communicable by them. Fresh cholera blood and filtered 'rice-water' fluid have, in different instances, been injected into the veins of dogs, cats, and rabbits, without producing any effects that indicate a specific poison, the results being only a temporary depression, and in some cases slight diarrhoea."

"The evacuations have also been thrown into the stomachs of these animals by Meyer and Marshall, but with uncertain results."

"In 11 instances no ill effects followed beyond temporary depression and slight diarrhoea. In 6, death took place in from 24 hours to 5 days. Mr. Marshall observes on these experiments, that 'equivocal rice-water discharges, blueness, coldness, cramps, tarry blood, and non-secretion of urine, are not in the catalogue of the effects, and so long as we know so little as a ground of comparison of the pathology of natural cholera in animals, we cannot draw safe conclusions from the phenomena produced by the administration of the cholera evacuations.'"

To resume, this flux is at once the consequence, and the evidence, of reaction; the last of a chain of phenomena, that may be considered either morbid or restorative, according to the point of view from which we regard them.

As it is the last of those, so it immediately

precedes, and is indispensable to, another set of phenomena, which may be considered more decidedly and purely restorative, inasmuch as they conduce to the renewal, as preliminary and necessary to recovery, of suspended functions.

Of the particular pathological process by which this flow from the bowels is instrumental in the renewal of those functions, and especially that of the liver, I need say little here, as it has been investigated in a preceding part of this work. (See Chapters I. and II.)

Without, however, insisting on the correctness of the views there propounded, offered as they were rather as suggestive than demonstratively true, I would submit the following more practical deductions, as in themselves simply consistent with familiar and admitted knowledge.

It is evident that owing, wholly or in part, to some change in the blood, certain functions have become suspended. Some further change therefore in that blood will be needed before they can be resumed.

Now this change consists mainly—and, as far as we can observe, wholly—in the transudation, by the bowels and skin, of its more fluid parts.

That, after the removal of those fluid contents, the blood should once again be found in a condition adapted for the extraction of special secretions, as those of the liver and kidneys, is evidently a fact

strongly in favour of the supposition that with those watery discharges was removed, either combined or free, a special poison; the previous presence of which in the blood was incompatible with the performance of those functions.

But, whether a special poison—and whether free or neutralized—be thus removed, it must ever be borne in mind, that, owing to the total suppression of two great functions, the hepatic and the renal, the blood must be contaminated with the conjoint elements, proximate or remote, of both bile and urine; whose removal, moreover, must form an important part of Nature's curative process. *Their* elimination at least can be scarcely considered hypothetical, or matter of conjecture only.

The extreme irritability of the whole alimentary canal may be attributed to two causes, operating in conjunction.

In the first place, the blood may have acquired a property specially irritating to the stomach and intestines. By the assumed elimination of a poison, or poisons, it would be gradually divested of this property, and the bowels would as gradually subside into quietude.

But another, and fully as intelligible a cause, is to be found in the well-known sympathy subsisting between the liver and the alimentary canal generally, so that the natural function of this conglomerate gland can never be interrupted without the

supervention of a proportionate degree of irritation in the intestines; familiar instances of which are found in some forms of diarrhœa, and in dysentery.

In the case of cholera however this irritation is intensified; not only by the *suddenness* of the biliary suppression; but by the concurrent, simultaneous, and equally sudden, arrest of the function of the kidneys; a complication unknown in any other disease, or under any other circumstances, and appearing in itself to constitute the very essence, distinctive and unparalleled, of cholera. In connexion with this subject I would refer the reader to the chapter on “The Nature and Properties of Bile,” especially to the remarks at pages 15 and 16.

Setting aside then the question of a special impurity to be removed, this fluid elimination is plainly needed to effect a *change* in the blood; such a change as will render it—not a healthy fluid, for that cannot be till after the resumption of all natural secretions—but a fluid, on which the liver, pancreas, and kidneys, can operate to produce those natural secretions.

For, to enable any secretory organ to exercise its proper function, and to extract its proper secretion, it is evident that we must furnish it with a circulation, a *pabulum*, possessed of certain specific properties: should those essential properties, whatever they may be, be wanting; or should others, foreign

or hostile, be introduced ; it is easy to conceive that that secretory organ will no longer be able to exercise, thereupon, its proper function ; and, therefore, that that function will stop. The necessity and advantage, moreover, of such a law will be readily perceived, without particular indication.

But this elimination, however indispensable, must needs be a very trying and exhaustive process ; and before that point can be attained, when the blood shall once more be reduced into a condition compatible with the functions of the now paralysed glands, the powers of life, already fearfully depressed, may well give way.

That critical consummation is without doubt signalized by a general and simultaneous, however faint and imperceptible, change, involving the whole economy. We can imagine the re-awakened sensibility of the nervous, and especially the ganglionic system ; the stronger movement of the heart, now preparing to propel a blood, from which the last traces of a mortal poison are disappearing ; the equable redistribution of that blood to all parts, with the contemporaneous relief of the congested viscera, and especially of the distended portal system ; together with that of the blood, the redistribution of the heat, similarly concentrated about, and hitherto confined to, the internal organs ; and lastly, the re-appearance of those familiar and much-desired secretions

(the bile, saliva, and urine especially), products and evidence of renovated life.

These physiological phenomena doubtless take place more or less simultaneously, though the objective signs by which we are apprised of their occurrence are, necessarily, in order *consecutive*. The gradual subsidence of vomiting and purging; the rising of the hitherto imperceptible pulse; the increasing warmth of the body, and of the air expired from the lungs; the diminishing thirst, and return of moisture in the mouth; the previous jaetitation and intolerance of covering, giving place to tranquillity, and a disposition to sleep, with the eyes elosed as in health; these are the chief of the earlier indications, that, to the anxious and watchful attendant, mark the dawn of recovery.

It is not till later that those more decisive tokens of safety, the secretions of bile and urine, are to be outwardly recognised. The latter, as coming from the bladder, is of course unmistakeable. Not so, however, with the bile. It is probable that the secretion of genuine bile by the liver is uniformly preceded by an elimination, on the part of the intestines themselves, from the distended and hypercholerized portal vessels, of a yellowish, or sometimes greenish, secretion—in fact, of a faetitious bile; sometimes in considerable quantity, and sufficiently conspicuous in the dejections; which, when that is

the case, acquire, before the true cystic bile appears, a faint yellow hue.

But the evidence of the presence of genuine bile is much more characteristic. As explained in a former chapter, after a temporary suspension of the liver's function, its renewal is marked by the passage into the bowels of a bile, that has been undergoing a process of concentration in the gall-bladder ; dark green, olive, or even black and tar-like, according to the period of its retention, and of a variety of other modifying influences before investigated.

With this genuine secretion—and not with the vicarious substitute alluded to, and which is but the herald and forerunner of the true—it is pretty certain that the renewed function of the kidneys corresponds ; but, no less assuredly, this is sometimes postponed. I have seen not a few cases, in which the presence of cystic bile in the discharges was unequivocal ; and in which the bladder was sounded day after day, with an anxiety only to be understood by those who know the vital import of the information sought for ; and in which the sufferers have eventually sunk with all the symptoms of uremic poisoning. Never, until this important secretion then be re-established, can we regard the battle as won, or our patient comparatively out of danger.

TREATMENT.—With respect to the treatment, the

first question that seems to present, is, the possibility of a specific remedy. By a specific for cholera, I would be understood to refer only to such an agent as, introduced into the body, either by the mouth or otherwise, would, by neutralizing or counteracting the poison, previously admitted into the circulation, cut short, at the onset, the chain of morbid phenomena we have been considering.

It need scarcely be said that, as yet, no such agent has been discovered. At the same time it is not difficult, I think, to conceive the possible existence of an antidote, that, introduced by the stomach, or directly into the circulation, should neutralize the poison already admitted, by the lungs, into the blood. It is even possible that any agent might have that effect, that should be simply more potent as a poison. The idea has but lately occurred to me; but in the event of cholera ever again making its appearance amongst us, I would suggest the trial, by hypodermic injection, of some of the more energetic and concentrated of our stimulants; as, strychnine, veratria, arsenic, iodine, bichloride of mercury, and phosphorus. Of course it would be advisable to observe due caution, commencing with the smallest proportions.

The idea of such an antidote seems also to receive some countenance, analogically, from what we know, by experience, of the poisons of serpents; which, in their pathological effects, exhibit, as already observed,

a remarkable affinity to cholera. In these bites, generally, both ammonia and ipccacuanha, if applied topically, and soon enough, produce an unmistakably "specific" effect; either by directly neutralizing the venom in the blood, or by inducing in the system an antagonistic condition, that renders it proof against the animal poison. In addition therefore to the stimulants above suggested, it would be interesting to ascertain the effect of these two remedies, both together and separate. The latter might be introduced in the form of emetine.

But no success is to be reasonably expected from the use of any antidote whatever, after the *secondary* chain of phenomena, described above as at the same time morbid and restorative, has been fairly established. Once that the hepatic and renal functions have been arrested, nothing but the completion of that chain can bring about their restoration. The pathognomic symptoms of cholera, whether brief or prolonged, severe or comparatively moderate, must run their typical course, if the patient is to live. To interrupt or oppose these, is to diminish the chances of recovery; to arrest them absolutely, supposing it in our power, would be certain and speedy death.

Supposing, for instance, we could undo all that Nature has been doing since that fatal moment, when the poison at length fairly got the better of the resisting power of the lungs. Suppose that we

could redistribute the blood and caloric that she has husbanded about the vital parts; at the same time recalling all other minor accompanying, or less obvious, pathological changes; does any one seriously imagine that such undoing would be a restoration to health. For my part, I believe the consequence would be instantaneous death. And I would further suggest, that a thus partial or imperfect work of defence, on the part of Nature, constitutes the true pathology of those rarer and more appallingly sudden cases, in which the victims sink, *without the pathognomic symptoms* of cholera.

Bearing then in mind the import, purpose, and even necessity, of these various pathological changes, known as the "symptoms" of cholera; regarding them as valuable and significant interpretations of Nature's working, and, at the same time, of her wants; the first practical inference hence derivable is, manifestly, the careful avoidance of every measure calculated to stifle or suppress these outward indications; which are, be it ever remembered, not the disease itself, but only its manifestations. To stop, *by direct means*, the vomiting and purging, would assuredly not be to cure cholera.

Having indicated, then, what is to be avoided in the treatment of cholera, we have next to consider what is to be done.

In conformity with the pathology here assumed, the *positive* indications of treatment become suffi-

ciently obvious. They may indeed be all summed up in the general principle of seconding Nature in her restorative efforts ; but, for practical purposes, they may be conveniently arranged under the two following heads :—

Firstly, to support the powers of life during the exhausting, but necessary, process of elimination by the bowels.

Secondly, to facilitate the restoration of suspended functions, especially those of the liver and kidneys.

The first of these indications is to be fulfilled by the exhibition of diffusible stimulants, regulated chiefly by the state of the pulse ; by draughts of cold water *ad libitum* ; by diligent friction, particularly of the extremities ; and by warmth to the feet.

With respect to the use of stimulants, I think their exhibition in excess is far more common, and, from the outward aspect of the disease, more likely to be practised, than the reverse. But, as already pointed out, a more active circulation of the blood, so long as it remains charged with a specific poison, is neither desirable nor expedient. We must remain content with the faintest indications of the pulse. It must also be remembered that the stomach is in a preternaturally vascular and congested state ; and the impropriety will be seen therefore of introducing into it strong and directly irritant

matters, such as Cayenne-pepper and raw spirit. Brandy, whiskey, or gin, however, sufficiently diluted, may be indifferently employed, according to convenience, and the patient's taste.

The unquenchable thirst is a constant symptom, and should be also regarded as a most valuable and emphatic token of Nature's working and her wants. It can in no way be better met, than by repeated draughts of cool spring water. I do not approve of ice; excepting when used for the reduction, to a refreshing and palatable degree, of the temperature of water. When ice, or iced water, is administered in any quantity, not only is the effect to lower still further the already fearfully depressed powers of life; but it is calculated to augment, rather than allay, the tormenting thirst. Indeed, the natural unfitness of ice, at any time, to allay thirst, must be a matter of familiar domestic experience.

I do not know that any part of the treatment is so essential and important as this constant and unlimited supply of a diluent in the shape of cold water. Even when in great part rejected by vomiting, enough is still absorbed to replace the waste by flux, and to dilute the fast thickening blood. In the course of many years, numberless cases have come to my knowledge of natives of India, who, attacked at a distance from medicine and aid, and intent only on the relief of present suffering, and, above all, of the intolerable thirst,

have sunk down by the side of lake or river, and found, in their limpid waters, the simplest, perhaps the surest, means of cure.

Soda-water, or lemonade, may be substituted for water, at the pleasure of the patient. Nitric acid, with honey or sugar, added to water, not only makes an agreeable drink, but seems to assuage the irritability of the stomach, and more distinctly to allay thirst ; it is also probably calculated, from its known specific tendency to act on the liver, to assist in promoting a renewal of the hepatic function.

Brandy, or other spirit, may be added to these draughts of cold water, according to the state of the pulse, and general condition of the patient. Hot drinks of any kind, however, should be scrupulously avoided ; not only as instinctively repugnant, but as likely to be more quickly rejected by the stomach. Where it is particularly relished, champagne, with plain water, or with soda-water, is one of the best of drinks. It is ever important, pathologically, to consult the instinctive preferences of the sick.

Should an extreme condition of collapse render desirable a more prompt and energetic stimulant effect, the following will be found a useful and simple combination :—Aromatic spirits of ammonia, one drachm ; camphor mixture and peppermint water, of each four drachms : to be taken as often as may be required, with half a wine-glass of

brandy. Cajeput oil, in doses of two or three drops, formed into an emulsion by the aid of gum, is also an efficacious and speedy stimulant.

Another important part of the treatment, and one that should never be omitted, consists in assiduous rubbing, especially of the extremities. I would repeat here what I have before stated in the pages of the *Lancet*, that for this purpose nothing is equal to the oil of Cajeput. The very odour seems to afford the patient satisfaction, and its efficacy in relieving the agonizing cramps is truly astonishing.

It is rapidly absorbed, and soon manifests itself in the breath, the urine, and, as I conclude, in all the fluids. To the facility and quickness with which it thus pervades the tissues, is doubtless attributable its special and singular virtue as a “diffusible” stimulant, rousing and sustaining the powers of life in a manner that no other external application can claim. When it is considered that the stomach can retain nothing, and that the absorbent power of its inner surface is also for the time annihilated, some idea may be formed of the value of a remedy that can be thus readily and speedily introduced into the system from without.

Its powers are conspicuously displayed in the young, and I have seen children apparently moribund rapidly revive under its use. In them the absorbents seem to act with greater vigour; and,

from the results of past experience, I should be inclined in future to employ it in the cases of very young children, at first, as an external application only, and without the co-operation of any other remedy; cold water to drink forming, under all circumstances, a part of the treatment. Indeed, until some slight degree of reaction is set up, the possibility of affecting the system at all through the medium of the stomach is, I think, very doubtful; and the impolicy therefore of needlessly harassing that important organ, and without some definite prospect of advantage, becomes sufficiently apparent.

In the first place a sheet of spongio-piline, sprinkled with the oil, should be laid over the whole abdomen. Four or five assistants, if available, should then be set to rub, one at each extremity. The patient will generally indicate, with eager impatience, those parts more immediately racked by the cruel cramps, entreating that they may be rubbed. The stomach should be rubbed at intervals, and then re-covered with the spongio-piline. There need be no fear of cold from exposure. The patient complains of suffocating heat, and will, generally, tolerate no covering whatever. In cold weather besides, a sufficient temperature may be kept up by means of a good fire; and bottles of hot water, cased in woollen socks, or other covering, should be placed at the feet, and between them.

Turpentine is the best substitute for Cajeput oil,

but immeasurably inferior. In ordinary colic, in chronic rheumatism, and in some forms of neuralgia, I am in the constant habit of employing the latter, and find no embrocation comparable to it in the relief of pain—I might almost say, of any kind. It undoubtedly possesses powerful antispasmodic properties, which appear hardly to have been recognised.

From what I have witnessed of its therapeutic effects, particularly in young children, I am satisfied that its diuretic action, when employed externally in the manner described, is also of a special and very powerful kind; and I need scarcely point out how this property also adds to its value as a remedy in cholera. I remember, notably, the case of an infant of about a year old, suddenly and copiously micturating under its use, when, from the condition of the child half-an-hour before, in apparently an advanced stage of collapse, I could have confidently affirmed that the bladder did not contain a spoonful of fluid. It has only to be added that recovery speedily ensued.

The second indication consists, as premised, in facilitating “the restoration of the suspended functions, especially of the liver and the kidneys.”

Should the work of elimination have been completed, and should the powers of life outlast that indispensable process, no doubt these suspended functions would, in the majority of cases, spontaneously revive, as the now depurgated blood com-

meneed again to flow through the secretory organs.

The powers of life, however, may not sufficiently endure, or the function of the liver (that of the kidneys being subordinate and dependent) may be but partially awakened ; and, after a feeble and sluggish effort, may again relapse. The juncture is critical, and most seasonable aid may be afforded to now nigh exhausted nature.

It need scarcely be said that, with the views here propounded, the use of opiates and astringents is utterly incompatible. I can conceive no circumstances under which the employment of such remedies would be admissible in any stage of cholera ; and I am convinced that, by the use of opium, thousands have been added to the number of its victims, who, if simply left alone, would have had a far better chance of recovery.

In place of a class of remedies so manifestly contra-indicated, we must look for one that will tend, if possible, to act upon, stimulate, and support the still torpid liver. The selection of calomel is obvious. None other can hold out equal hope of specific and certain effect. Amidst vaunted remedies innumerable, an immense preponderance of testimony is already in its favour ; but universally its good effects have been counteracted, paralysed, rendered in fact almost impossible, by a conjunction with opium.

But, in giving calomel, there are two other advantages, secondary only in importance to that of its pre-eminent cholagogue property, and which, though I have had them always in view, do not appear to have been prominently brought to notice.

The first of these consists in its being the only medicine—that is, of those that one would think of prescribing under the circumstances—that is not immediately rejected from the stomach. I am in the habit of washing down each dose of calomel with some aromatic or stimulant draught, such as that before recommended; and have noticed that, though the liquid may be returned by the stomach, the powder remains. This surely is a very important and valuable property. I suppose it to be mainly attributable to the ponderous nature of the calomel, but also in part to its quality of adhering to a mucous surface.

The other advantage referred to consists in a direct sedative effect on the stomach and whole nervous system, calming and soothing the morbid irritability of both. Not only is the vomiting moderated, but so also are the sympathetic cramps. This *sedative* effect may be only a secondary one, consequent on the primary *stimulating* effect on the liver, such being a general law of cholagogue action, as illustrated in a former chapter. Be that as it may, that such sedative effect, at least in some degree, does always ensue, repeated experience has

satisfied me. And, further, I need scarcely point out that it differs, *toto cælo*, from the so-called "sedative" effect of opium. To soothe is not to benumb—to calm irritation is not to destroy sensibility.

I do not imagine that the calomel can possibly produce its specific effect on the liver, till the blood has been depurgated, and commences again to flow ; till, in other words, the conditions and machinery of secretion are re-established in that great conglomerate gland. Notwithstanding this belief, I give calomel from the onset of the disease ; and, it may be asked, "For what object, if its specific action be not possible?" To this I would reply : firstly, that it is impracticable to say how soon that point of depuration may be attained, when the liver will be in a condition again to act ; secondly, that, besides its apparent sedative effect on the stomach and nervous system, its otherwise inert presence can do no harm, and it will be ready there, when wanted, to exert its proper effect.

When an opportunity is afforded of treatment at a comparatively early stage, ten-grain doses are sufficient, repeated as quickly and as often as circumstances may seem to require, until decided reaction is apparent.

The dawn of recovery is recognisable by the gradual subsidence of the vomiting and purging, and the rising of the pulse. The watery motions

are, after a time, replaced by pretty consistent dejections of seemingly black bile. By transmitted light, however, these will be found of an intense, but bright, olive, subsequently passing through successive shades of green, till the familiar yellow tinge is attained. The restoration of the renal function follows, generally immediately, but sometimes at a considerable interval, that of the hepatic. As far as my experience goes, it never precedes it. Nor, until it is re-established, can we consider the danger as past: it is, therefore, looked for with intense anxiety. Under circumstances of unusual protraction, a blister to the loins will be found a powerful adjuvant, and should be resorted to whenever this important issue is doubtful or much delayed.

Blisters and sinapisms, especially to the calves of the legs, are often resorted to, chiefly, as I conclude, with a view to stimulate the circulation, and rouse the sinking powers of life. That they have such a tendency is very probable; but I think an equally important result obtains in their derivative, or counteractive, effect in diminishing the gastric irritability, and the distressing vomiting thence arising. When I was at Futtehpore and Kirwee, during the Indian mutiny, a form of remittent fever prevailed, of which one of the features was vomiting, uncontrollable by any of the ordinary means, including sinapisms to the epigastrium. Having lately treated

with perfect success a most unpromising case of chronic synovitis of the knee, by the use of setons in the arm, and thus impressed with the advantage of distant counter-irritation, I was induced to try, in these cases of fever, the effect of a very small blister, not exceeding the size of a crown piece, to the calf of one leg. The result more than fulfilled my expectation ; for, with the earliest effect of the blister, not only did the vomiting commence to subside, but there appeared at the same time a decided mitigation of the other febrile symptoms. So that the application of a little blister to the leg became, on the part of the hospital assistants, a matter of course in every case of fever, and they were very numerous, in which there appeared any symptom of vomiting. I have since used the same remedy, with invariable success, in many cases of obstinate vomiting, not attendant on fever. The gastric irritability in cholera is a symptom more distressing to the patient than even the flux itself, and any means, therefore, of alleviating it becomes an important auxiliary in the treatment. I do not think the application of large blisters to both legs necessary, excepting perhaps in a condition of extreme collapse ; but the effect of a small one, as above described, can never be otherwise than harmless, and should, therefore, be resorted to in every case.

Of the two indications of treatment above laid down, I consider the *first*, of which the sole object

is to sustain the powers of life, as not only by far the most important, but as, at the same time, the only one imperative. The *second*, that of directly arousing the dormant secretions by means of calomel, is to be regarded as subordinate in comparison; and it will also be understood that, though I am inclined to look upon calomel as the surest remedy of its class, I believe that tartar emetic, ipecacuanha, or even common salt, may be employed with the same object, and on the same general principles of "rational" treatment. For emetics, as before observed in speaking of the treatment of dysentery, are all more or less cholagogue in their action. It is also probable that castor-oil, and the whole family of purgatives, might be used with a similar intent. But to insure the cholagogue action of a purgative, it must be given in smaller doses. Thus, castor-oil, in doses of a table-spoonful, frequently repeated, has been found very successful in the hands of Dr. George Johnson.

From these remarks it will be seen that I am by no means wedded to a particular remedy. And though, from the results of personal experience, I incline to the use of calomel, I should be quite ready to believe that, in other hands, castor-oil, or tartar emetic, or table-salt, would answer quite as well.

Nor should I hesitate to employ any one of them myself, believing that I might do so in perfect consistency with the pathological views I entertain, and

that I have in the course of this work endeavoured to lay before the reader.

But, after all, with respect to the treatment of cholera, it can scarcely be doubted that a certain number of cases—especially among those of earlier occurrence—in any given district, town, or still smaller community, as in ship, jail, or workhouse—would, under any treatment, prove fatal, in the present state of our knowledge. It must always be a contest between the intensity of the poison on the one hand, and the natural “vis medicatrix” on the other. Should the former, therefore, surpass a certain point, or the latter fall short, human skill will not avail to turn the scale. Still we must never remit our attention. There is no disease in which is so often and strikingly exemplified the familiar observation that “while there’s life, there’s hope.” Numbers, indeed, have rallied and recovered after life had been pronounced extinct.

In connexion with this feature of apparent death, I cannot help taking the opportunity of expressing a very painful suspicion that, more particularly in India, interment does sometimes occur, not only whilst the spark of life still lingers, but while there yet remains a chance of recovery. An exaggerated notion—not certainly on the part of medical officers, but on that of the authorities and of the public generally—of the danger to the living from the results of decomposition, leads to this selfish and

unseemly custom of hasty interment. Amongst ourselves in India it has been doubtless in a great measure founded on an observance of the usage of the country. But it must be remembered that the possibility of being buried alive is not by any means regarded by the natives with the same horror as by us ; it being a familiar custom, especially in Bengal, to stop, with the mud of Ganges, or other holy stream, the mouth and nostrils of those supposed to be dying. I would hope that the attention of the Indian Government will be directed to this painful subject, and that some legislation may ensue to restrain the present unseemly practice, with its hideous contingency ; at the least with respect to those attended by our own medical officers.

No body should be allowed to be covered up with a cloth, or placed in a coffin, or otherwise *treated as a corpse*, till at least twelve hours have elapsed from the moment of *apparent* dissolution. The simple extension of the period of actual interment would evidently be insufficient ; for, to enclose a living body in a coffin, or even to diminish the access of air by covering the mouth and nostrils with a cloth, would in many cases be quite enough to sever the precious and slender thread of life.

In the hottest weather in India, the results of decomposition are not, under any circumstances, unpleasantly perceptible in less than twelve hours ; whilst, by simple deposit in an airy and shaded

apartment, with a moderate use of chloride of lime, a body may be preserved for a much longer time. Medical officers in India, therefore, would do well to discountenance the fears entertained by the non-professional public on this head. Some moral courage is ever needed to stem the various currents of popular prejudice; but "*magna est veritas, et prævalebit*," and justice in good time will surely be done to the courageous assertor of truth.

Nothing has yet been said of the treatment of the premonitory diarrhœa, or other derangement of the bowels, so commonly met with during the prevalence of cholera. The indications of treatment will be obvious, when we consider that a struggle is signalized between the system and a noxious influence, and that the diarrhœa is, in fact, a conservative process. The best and most rational treatment, therefore, of ordinary diarrhœa, whether bilious or *acholic*, will be the best here; not by opiates or astringents, but by the employment of a purgative, giving a preference to such as may be considered more decidedly cholagogue. If not contra-indicated by some of the circumstances enumerated in the chapter on "*Cholagogues and their action*," calomel, in a single ten-grain dose, which will seldom require a subsequent draught to carry it off, will still be our surest auxiliary. Castor-oil, however, or rhubarb with magnesia, will commonly answer quite as well. It will also

be found safer to prescribe a full and sufficient dose ; the effect of one insufficient being to cause pain and increased irritation, often with a precipitation of the more dreaded and fatal evil.

It will not be necessary to expatiate on the importance and advantage in all cases of early treatment, this point appearing to be now pretty generally recognised.

I shall refer but very briefly to the “secondary fever,” or other sequelæ of cholera, chiefly as I have nothing new to suggest with regard to the treatment, which, as is well understood, must be adapted to symptoms, as these may present themselves, and according to the condition, whether sthenic or adynamic, of the patient.

But, with respect to these ulterior consequences, I cannot help observing that, in a majority of instances, I believe they are owing at least as much to the remedies that have been employed as to the previous paroxysmal disease. If, instead of carefully observing Nature, and lending an occasional helping hand discreetly and opportunely—if, instead of acting thus, we should have embarrassed her with needless and clumsy aid, clogging her well-meant endeavours with over-potent drugs, we may well look for this excessive reaction, with a proportionate degree of secondary fever. Indeed, I believe it may be advanced as a positive truth, that in those cases where recovery has been nearly or wholly

spontaneous, and such instances in India are not uncommon, subsequent febrile symptoms do not occur, or at least not so markedly as to call for active treatment.

When the elimination of the poison has not been thus unduly arrested or impeded, not only the elements of urine, but also the *uncombined* elements of bile, may be supposed to have been in great part removed ; and thus are anticipated and prevented those “secondary” effects, themselves often fatal, and which I am disposed to regard as the joint results of cholæmic and uræmic poisoning. The latter is a familiar pathological phenomenon, on which I need say nothing. With respect to the former, I must content myself with referring to what has been observed in a previous chapter, to the effect that, though bile itself, when dispersed through the tissues, appears to have little or no injurious influence, its *uncombined* elements, on the contrary, are eminently poisonous, and do often prove rapidly fatal to life.

Before concluding the subject of cholera, it may be worth while to notice an opinion that has been advanced, to the effect that cholera is but an aggravated form of dysentery, or even of diarrhœa. But it may be sufficient to recall the fact, that dysentery itself may be so severe as to prove fatal in two days or less, without assuming the slightest outward or objective resemblance to cholera ; whilst,

on the other hand, the mildest cases of even sporadic cholera can never be mistaken for dysentery. I have certainly known patients with dysentery attacked by cholera; but in such cases anuria was one of the earliest indications, and the characteristic symptoms of the one disease are at once replaced by those of the other. In its pathological conditions, and for the purpose of illustration, cholera may be said, by a figure of speech, to be dysentery intensified by the addition of anuria; but this addition is more than enough in itself to constitute a distinct disease.

CHAPTER IX.

ON SANITARY CONDITIONS.

It should be borne in mind that our knowledge and estimate of what we term “sanitary” conditions must ever remain very partial and imperfect: in fact, it is limited to a perception of such conditions only as are recognisable by the outward senses—or rather, by the single sense of smell; for though we do, and none the less authoritatively, pronounce on what is pure or impure, by the mediation of sight alone, it is evident that, when we happen to be right, it can only be inferentially, or by the force of association.

It is evident moreover that an infinity of the most baneful influences to which humanity is subject, are of a nature so subtle as to elude the keenest perceptions of sense; so that their very existence is only deducible *from their effects*. Those floating poisons that emanate from the persons of the sick, in plague, small-pox, and other disorders decidedly contagious; the “malaria” gene-

rated by paludial soils, rendering ague endemic in whole districts; or those still more deadly influences prevailing at the bases of tropical hills, girt with a dense belt of vegetation, and where to sleep a single night is familiarly spoken of as "certain death:" these are all equally unseen and unfelt—and yet they undoubtedly *are*. Of some we can with certainty indicate the source, and, by draining a marsh or levelling a forest, effect their annihilation.

But it must be remembered that, in these instances, it is only by the manifest relation of cause and effect that we have this knowledge. By how many other noxious influences are we surrounded, of whose individual existence we have not so much as a suspicion—*even by their effects?* Assuredly,

"There are more things 'twixt heaven and earth
Than are dreamt of in *our* philosophy."

Many of these are the inevitable growth of an artificial condition of society, in which Nature's original provisions for our well-being, abundantly liberal and sufficiently obvious though they be, have been habitually and systematically disregarded. The greatest source of such impurities is probably the human body itself—that is, of impurities specially noxious to humanity; not simply by the consumption of oxygen, and consequent deterioration of the element in which we exist, but by the evolu-

tion of a variety of impurities, that do not admit of identification by sense, much less of examination by science. Unhealthy persons probably contribute a larger share of such noxious emanations than those in comparative health. Of such, the invalid who daily bathes, and is clothed in fine linen, may be a more fertile source than the artisan or labourer, whose ablutions are hebdomadal, or a mere subject of conjecture. And yet, judging by the eye alone, we should probably arrive at quite an opposite conclusion. But washed or unwashed, sick or well, it is certain that, in spite of every sanitary precaution that we are *now* engaged in, and of every projected "sanitary" measure that we may succeed in establishing, each member of the population will still continue to furnish his quota of contamination. What precaution are we taking—which one of our sanitary projects is directed to the prevention or counteraction of this great inherent source of impurity?

By these remarks it is far from my intention, even by implication, to underrate the value and importance of personal cleanliness, but to point out that its most scrupulous observance will not prevent the evolution, from each living organism, of impurities, intangible indeed, but no less certainly existent and noxious. Where there is a sufficient atmosphere surrounding each individual focus of impurity, the result becomes inappreciable; or,

more probably, a process of actual chemical neutralization ensues. When, however, there is not this sufficient medium of dilution or neutralization, the consequence must be an air permanently vitiated and unwholesome. And, be it again noted, this must be the inevitable result, in spite of the most absolute conditions of outward and visible purity, in respect of dwellings, clothes, or persons. Not even the principle of a certain "cubic space"—as has been hitherto assumed and acted upon, especially in the accommodation of our troops in various parts of the world—but a certain *area per man* must be allotted, if we would secure the *one* great and only indispensable condition. Without it, other precautions can prove at most but palliative or mitigatory; with it, others may be, to an almost incredible extent, neglected, or at least left to individual sense and instinct. We may institute the most perfect system of sewerage; lodge our poor in houses, very models of comfort and convenience; enforce daily ablution and clean linen by Act of Parliament, till a sanitary millennium be established, and visible dirt and bad smells shall have become mere things of tradition; and notwithstanding, after all is done, it is doubtful whether we shall be much nearer our object, or have effected any material reduction in the rate of sickness and mortality. Certainly not, if in each community an average share of our mother earth's surface—man's hereditary and in-

alienable right—be not at the same time recognised and allowed.

In fact, what is called “sanitary science,” in its present theory and application, is a scheme to reconcile the enjoyment of Nature’s choicest blessing with a disregard of Nature’s plainest and most imperative law. If this be true, it is confessedly a very ugly truth, and that one would gladly shut out, if possible, from view. It is so very discouraging to think that all the thought, and labour, and money, now being expended, may, after all, go but a little way to the fulfilment of the great necessity of the hour. Shall we then close our eyes, and go on, trusting that so much energy and good intention cannot but be in the end rewarded? Would it not be wiser to confront the evil, pause awhile, and consider what may be, after all, the true solution of this greatest social problem? When, as already said, we shall have purified our thoroughfares, dwellings, and persons, till nothing remain to offend the most fastidious taste, does any one seriously expect that public health will approach a fair average standard, so long as those darker blots, significant of overcrowding, are allowed to appear on the maps of our towns and cities?

Were I addressing the public generally, I might be expected to enter into somewhat fuller detail on this subject, and to show, that the incompatibility of health with overcrowding, even under the most

favourable conditions, has been proved beyond a possibility of question, by a vast concurrence of testimony and experience. It would be sufficient to cite the familiar example of our household troops, who did, if they do not still, furnish a striking proof of the prevalence of this natural law. But every member of the profession knows that, so far from advancing anything new or strange, I am but insisting on the necessity for keeping steadfastly in view what has already become an established axiom of social economy.

I would therefore only further take the opportunity of pointing out what has always appeared to me a dangerous fallacy, not only in the practical application, but in the very theory and conception of this axiom ; and that is the notion, that the space required for human beings can be estimated by cubic measurement only—*without any reference to area.*

I believe I am correct in stating that, in all our public institutions, our prisons, barracks, and hospitals, this principle of cubic measurement is the only one officially recognised and acted upon. It needs, however, but little reflection to understand that, if a hundred soldiers are sleeping with their cots almost in contact, the injurious result will be but slightly mitigated by the circumstance of the apartment being a very lofty one. Even suppose that lofty barrack unroofed—open to the very sky—with-

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out calling in science to our aid, will not common sense suffice to teach that (in the supposed absence, of course, of any ventilation) the atmosphere would be, by the morning, intolerable to any one entering from without; that, whatever might be the condition of the air *above*, there would be a dense stratum below, loaded with the mephitic exhalations of a hundred sleeping men?

But, in place of a lofty or unroofed barraek, imagine a second, third, or even fourth tier of human life, above the first. What might we not then look for—especially in that lower stratum? But further yet—picture a vast number of such buildings, ranged in narrow streets, and with their tiers of sleepers, and we do but realize the actual condition of parts of our great manufacturing towns.

Under such circumstances what can even ventilation effect, but the exchange of one's own bad air for that of a neighbour? In winter, moreover, the little warmth accumulated in the dwellings of the poor is too precious even for ventilation.

But it may be a matter of doubt, and this I offer by way of suggestion only, whether an unlimited supply of the purest air would suffice, in the absence of an adequate terrestrial area. We know, or assume that we know, the normal chemical constitution of the atmosphere; but what other unknown conditions, and totally independent of the atmosphere, may there not be? To say nothing of electric

influences, there are others, apparently as independent of the atmosphere as are the rays of heat and light. In the event of cholera approaching us from the north, does any one suppose that all that would be necessary to stay its advance would be a wind, strong and steady enough, from the south? It is impossible to doubt the existence of this and other such deleterious influences; and which, in our ignorance of their nature, we must be content to term "tellurial." And if there be such *deleterious* agencies, of whose *entities* we are assured by their effects, may we not infer the analogous existence of *beneficial* influences as well, indispensable for counteraction or neutralization; if not derived from, at least communicated through, the earth, and necessary to the well-being of the creatures on its surface? A little knowledge may not be always a dangerous thing; but in every attempt to utilize that knowledge we have, let us remember how infinitely more remains beyond; and that, in all our pride of intellect, we may yet be on the mere threshold of science.

This idea of the necessity of a certain superficial *area per man*, on the ground alone of unknown and beneficial tellurial conditions to be thereby derived, might well appear too crude, or even too fanciful, for serious consideration. But I trust its necessity will be sufficiently apparent on grounds more substantial and intelligible. I have already supposed the case of a number of soldiers sleeping in a lofty,

but roofless, barraek, on a still night, and in the absence of artificial ventilation. But, it may be asked, why not have ventilation, which—at least in a barraek sufficiently detached from other dwellings—would be adequate to preserve the air in a wholesome condition? To this I reply—and for confirmation I appeal to every medical officer of experience—that however feasible in theory it may sound, it has never been found possible in practice, chiefly for the following reasons:—

Firstly, people of the lower class, and soldiers no less, have an invincible repugnance to drafts, or to anything in the shape of ventilation; and will have recourse to every possible device, by interposition of articles of dress, or other substitute at hand, to close every discoverable chink, hole, or creanny.

Secondly, in cold weather the amount of air necessary from without, too far reduces the temperature within. This consequence is so familiar, and besides so obvious, as to need indication only.

Thirdly, in hot weather, ventilation is simply impossible, because *the air without is of the same temperature as that within*; and I need scarcely point out that, in the absence of wind, there can be no natural ventilation *without difference of temperature*. In winter, when our apartments are artificially warmed, one has only to put one's hand to the key-hole to be satisfied of the abundant ventilation we then, *nolentes volentes*, enjoy. On a still summer

night, on the contrary, a candle will burn even at an open window, with scarce a flicker. We may conceive then the number of nights in a tropical year when ventilation is a physical impossibility. And on such nights imagine double rows of men, sleeping, as they do in every barrack in India, with three, or at the most four feet, between their cots. No matter what the height of the barrack, let the visiting officer enter towards the close of one of those stifling nights, before every door and window has been thrown open to court the faint morning breeze, and he will need no chemical tests to estimate the condition of the gases within.

I have often heard it urged by non-professional men, who may be excused for their imperfect acquaintance with scientific principles, that the absence of ventilation, on hot nights in India, is now compensated for by the use of punkahs, or large fans suspended from the ceiling; *as if impure air could be made any purer by simple agitation.* The thermantidote, however, a machine for propelling air into a room by means of fans revolving on an axis, might be used with undoubted advantage on hot nights; but on cold nights, as already observed, such free admission of the outer air would be intolerable.

I trust that I have now said enough to show, on practical grounds alone, that, for the purpose of allotment, whether in jails, barracks, or dwelling-

houses, of sufficient space, the only safe and reliable estimate is one based on *superficial* measurement. If we would establish perfect sanitary conditions, we must commence by allowing to each man so many square feet of the earth's surface. In other words, the number of persons sleeping in any building should bear a due proportion to the number of square feet on the ground-floor.

The number of cubic feet that should be allowed each inmate of a prison or barrack is laid down, by writers on the subject of health, as not less than 500 or 600; and this estimate is, I believe, generally recognised in Government buildings. Assuming its adequacy, we should have, in an apartment of 11 or 12 feet high, an area per man of about 50 square feet.

Thence we should also conclude that in buildings consisting of a ground-floor only—and nearly all our barracks and jails in India are so constructed—there should be allotted this individual space of *not less than 50 square feet*, without reference to height. But, and in India especially, such an *area* is manifestly insufficient.

In tropical countries, as protection from a powerful and vertical sun, all public buildings, though consisting of a ground-floor only, are necessarily lofty; rarely, perhaps, less than 20 feet. If, therefore, in such a building, a cubic estimate of 500 feet were recognised as sufficient, the area for each man, in-

stead of 50, would become only 25 square feet. Imagine, in such a climate, rows of men, each in a space of seven feet by three and a half! Would the twenty feet above compensate for this want of room below? Not of course that I would have it inferred that such close packing does ever actually occur—excepting perhaps under canvas. But I make the supposition to show the utter inadequacy of merely *cubic* measurement.

In temperate climates again, public buildings rarely consist of less than three floors—often of more. But, as already shown, we cannot have these successive tiers of sleeping human beings, without baneful deterioration of the air, especially in the lowest tier, *unless an adequate area be provided*.

The following data from Dr. Baly's Official Report on Cholera, page 37, are strikingly illustrative of the views advanced above:—

“Of 472 deaths from cholera in London, during the months of January and February, 1849, 257 occurred in the Pauper School at Tooting, the Female Refuge in the Hackney Road, Warburton's Lunatic Asylum (Bethnal Green), Millbank Prison, and a few workhouses. Of 10 deaths in the extra-metropolitan part of Middlesex in the same months, 7 occurred in the Edmonton Workhouse; of 29 in Hertfordshire, 12 occurred in the County Gaol and Union Workhouse; of 17 in the East Riding of Yorkshire, 12 occurred in the Workhouse at How-

den ; and of 61 in the West Riding, 16 occurred in the Wakefield House of Correction, and 7 in the Union Workhouse at Selby.

“ At the end of the year 1849, again, the deaths in large institutions preponderated at many places. Thus in November the only deaths in Taunton were 58 in the Union Workhouse, there being only 168 deaths in the whole county ; and the only deaths in Hertford were 7 in the County Gaol, the whole number in the county being only 23.”

All our care, labour, and money will be but thrown away, so long as we leave unremedied that first and great capital source of misery, crime, sickness, and death—OVERCROWDING.

APPENDIX.

Note.—The following is the article from the *Lancet* of October 7th, 1865, referred to in the Introduction; and which I have been induced to reproduce here, chiefly with a view to show that, though the publication of this work has been delayed, similar views respecting cholera, its nature, treatment, and inevitable progress, were entertained by myself, and promulgated in a condensed form, upwards of a year ago, and whilst an actual invasion by the disease was still a matter of conjecture only:—

ON THE PATHOLOGY AND TREATMENT OF ASIATIC CHOLERA.

THE near approach of cholera to these shores, the virulence that has marked its progress through the countries it has already visited, and the possibility that we may shortly be called upon to do battle with it here at home, afford together matter for grave consideration.

I am induced to bring before the notice of the profession a mode of treating the disease which my own experience has found eminently successful; and I trust that, under the circumstances of possibly coming danger, no apology will be needed. At the same time, I am fully sensible that, in demanding that a new and special treatment shall be subjected to the impartial test of general

experiment, a reasonable prospect of success should by the originator be afforded.

During twenty years passed in India, I have had repeated opportunities of observing the disease, and have been led to the conclusions (most important if true)—

Firstly. That no sanitary precautions, in the present state of our knowledge, will avail to stay its onward course, which proceeds by mysterious, maybe immutable laws, surely, irresistibly; nor to moderate its virulence in individual cases, the first few proving generally fatal, and subsequent seizures becoming gradually milder as the disease seems to exhaust itself.

Secondly. That sanitary measures tending to raise the standard of public health, and thereby to counteract a predisposition in individuals to the disease, will avail very much to limit its ravages, by reducing the total number attacked.

Thirdly. That a "rational" may take the place of an "empirical" treatment; the latter term of course being taken in its legitimate, not an injurious sense.

On the first two points time and space will not permit of my dilating at present; therefore, and not without reluctance, I limit my attention to the last.

In considering the treatment, it will be manifestly convenient to exclude such cases as have already passed into a moribund condition, as also those in which the sequelæ of the disease have supervened.

In this formidable malady the functions of the liver, kidneys, salivary glands, and (presumably) the pancreas are suddenly and absolutely arrested. So that were I asked to state, as succinctly as possible, in what cholera seems to consist, I should say, "In the total suppression of some secretions, and the more than proportionate excess of others." Impressed with this view of the pathology of the disease, I was led at an early stage

of my experience to regard the profuse discharge from the mucous membrane as indicating Nature's principle of cure, by eliminating from the system the *poison*—or by whatever other term we may designate the noxious condition—on the existence of which the disease depends. Acting upon this supposition, and with that belief still further strengthened by a consideration of the already fatally inert condition of the liver and kidneys, I very quickly abandoned the conventional treatment by opiates and astringents. If, I argued, this deadly torpor of the liver and kidneys be indeed the proximate cause of the mischief, the “*fons et origo mali*,” perhaps the very essence of the disease itself, and if again these abundant evacuations be, as all analogy would lead us to infer, a desperate but curative operation of Nature, surely the very way to increase the existing evil on the one hand, and to paralyse these salutary efforts on the other, would be to prescribe such a drug as opium.

Discarding then, with all its class, a remedy so glaringly inappropriate, I had to find one that would, on the contrary, revive the suspended functions. The selection of calomel was obvious. None other could hold out such hope of specific and certain effect. Amidst innumerable vaunted remedies, an immense preponderance of testimony was already in its favour; but, universally its good effects had been counteracted, paralysed, rendered in fact almost impossible, by a conjunction with opium.

The following, then, were the indications of treatment I kept in view:—1st. To restore the hepatic and renal functions. 2nd. To assist—assuredly not to check—Nature in her exhausting, but necessary, process of elimination by the bowels.

For the fulfilment of the first indication I relied on calomel given in ten-grain doses, and repeated every half-hour, or even every quarter, according to the intensity of

the attack. I have also found, at a later stage, a large blister to the loins have a surprising effect in directly stimulating the kidneys to action.

The second indication—that of supporting Nature in her work of elimination—is to be fulfilled by diffusible stimulants, regulated by the state of the pulse; draughts of cold water *ad libitum*; hot-water bottles to the feet, and diligent friction of the stomach and extremities.

Whenever, therefore, I have been called to a case sufficiently early, I have invariably had recourse to the following measures:—Ten grains of calomel washed down with a stimulant draught are at once administered, and repeated as quickly and as often as may be requisite. It is surprising, by the way, considering the violent and continued retching, how little, if any, of the powder is brought up again. Hot-water bottles are placed at the feet, which themselves are wrapped in flannel. Three or four attendants are employed in assiduously rubbing the abdomen and extremities with cajeput oil. The almost instantaneous result obtained by this rubbing process, not only in relieving the agonizing cramps, but in rousing and sustaining the failing powers of life, is truly astonishing. There is no embrocation that I know comparable to it in allaying pain—I might almost say of any kind—for I am in the daily habit of prescribing it in colic, chronic rheumatism, neuralgia, &c. It is rapidly absorbed, and manifests itself in the breath, the urine, and, as I conclude, in all the fluids. In the intervals of rubbing, the whole abdomen is covered with a sheet of spongio-piline sprinkled with the oil.

The insatiable thirst, affording another instructive and emphatic token of Nature's working and her wants, can in no way be so well met as by copious draughts of cool spring-water. Even when in great part rejected by vomiting,

enough is absorbed to replace the waste by flux, and to dilute the fast thickening blood.

The dawn of recovery is recognisable by the gradual subsidence of the vomiting and purging, and the rising of the pulse. The watery motions are, after a time, replaced by pretty consistent dejections of seemingly black bile. By transmitted light, however, these will be found of an intense but bright olive, subsequently passing through successive shades of green, till the familiar yellow tinge is attained. The restoration of the renal function follows, sometimes at a considerable interval, that of the hepatic. As far as my experience goes, it never precedes it. Nor until it is re-established can we regard the battle as won; it is therefore looked for with intense anxiety. As already stated, a blister to the loins is under these circumstances a powerful adjuvant, and should be resorted to whenever this important issue is doubtful or delayed.

During the present season an aggravated form of diarrhœa has been prevalent in Nottingham, presenting, in sporadic cases, the most prominent features of Asiatic cholera, such as the rice-watery stools, severe cramps, and suppression of urine. The three following may be taken as illustrative. In each the treatment was that already described: in one case with such speedy success that the patient was able, after an interval of thirty hours, to travel from here to London; and in another to attend on the following day to his duties in the Assize Court.

CASE 1.—Mr. G——. I was called early on the morning of July 20th to see this gentleman, and found him in great pain, the abdominal muscles and those of the extremities being contracted by violent spasms. He had been repeatedly purged during the night, the stools of the consistence of thin gruel, and of the colour of parchment.

Nothing could be retained on the stomach, but came away accompanied by colourless mucus. Tongue white and dry; pulse 100, small and wiry. The rubbing with cajeput oil was commenced immediately, to the intense satisfaction of the patient, who said that the very smell "seemed to do him good," and spongio-piline was kept ready to be laid on the abdomen. Ten grains of calomel were washed down with the following draught:—Aromatic spirit of ammonia, one drachm; camphor mixture, peppermint water, of each four drachms: to be taken with half a glass of brandy. No recurrence of vomiting took place after the calomel was swallowed.—Twelve noon: He had been directed to repeat the calomel in the event of the vomiting returning, and to take the draughts every two hours, or oftener, according to circumstances. He is now quite free from pain; has not vomited, and has had one scanty motion. Some slight spasms of the abdomen and one hand were relieved by the cajeput oil. Surface warm and perspiring; pulse 90, full; tongue florid, and white at base. To have some beef-tea; draughts to be taken only if required.—Seven P.M.: Very sore from the effect of the cramps; bowels not moved; has passed a little high-coloured urine, being the first during the last thirty hours. Feels quite comfortable. Nil.

July 21st.—Feels quite well, but weak. His presence at the Assize Court being indispensable, I could not prevail on him to remain at home. With a view to a gentle action of the bowels, the following was prescribed:—Rhubarb powder, one scruple; carbonate of magnesia, two scruples; powdered ginger, ten grains; cinnamon water, one ounce.

22nd.—Had three motions after the draught yesterday, very dark-coloured, and this morning one of a more ordinary colour. Feels on the whole very well.

23rd.—Found my patient this morning, as he says,

better and lighter than he has felt many a day. He was apprehending a fit of gout, to which he is subject, when this attack commenced. This was the last visit I paid him.

CASE 2.—Mr. B—— was taken in the middle of the night of the 28th of July with purging; has just passed one of the true rice-watery character. Pulse 120, very small; face shrunk, and bedewed with a cold perspiration. No urine has passed since yesterday. Colicky abdominal pains, with cramps, not very severe, of belly and legs. Great thirst; tongue white and furred. He partook of brandy-and-water during the night for the purging, but latterly the stomach rejects everything.

The treatment was precisely as above recorded, except that a second dose of calomel was needed to subdue the symptoms. On the following morning he was so far recovered as to require no further treatment, and the same afternoon returned to his residence in London.

CASE 3.—Mr. B—— has been suffering from slight diarrhœa for some days past. He states that the motions were dark-coloured. This morning (August 13th) he vomited, and since has been able to retain nothing on the stomach. The purging, too, has been incessant, with painful cramps of the abdomen and extremities. The stools are ash-coloured. No urine has passed since yesterday, and the bladder is evidently empty. Countenance much shrunk. Pulse 120, scarcely perceptible; clammy sweat.

The treatment and progress of this case so much resembled the others as to render detail needless. A tendency to *bilious* diarrhœa, however, with colicky pains, remained for two or three days afterwards. During this time the spongio-piline, wetted with cajeput oil, was re-

tained on the stomach. At the end of three days he was convalescent.

The cajeput oil undoubtedly possesses powerful antispasmodic properties, which appear hardly to have been recognised. In recommending its use in the manner described, I do so entirely on my own personal experience, not having seen it used in a similar manner by others. But as my observations upon it, as well as upon cholera generally, have been made in India, where different physical and social laws prevail, I shall be glad if any of my professional brethren should be inclined to adopt the treatment I have proposed, and kindly to record the result of their experience in the columns of the *Lancet*.

THE END.



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